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FURTHER REMARKS ON THE ORES OF ENGELS, CALIFORNIA.[1]

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PREFATORY NOTE.

In the year that has elapsed since the completion of our paper[2] on the Engels ores, a considerable amount of work has been done in this laboratory upon other ores possessing in one respect or another features sufficiently similar to those exhibited at Engels to throw some additional light on interpretations applied to that district; and although our Engels collections have not been deliberately or systematically re-studied, occasion has frequently arisen to refer to Engels material in connection with ideas suggested by the other regions. There has appeared, moreover, a discussion of our paper by Professor Tolman,[3] to some parts of which it might seem proper to make answer. Neither of these circumstances, however, might justify us in asking further attention for this particular subject if its applications were practically confined to the occurrences in this relatively unimportant copper-producing district, but since at least two matters believed to be of fundamental consequence and of extended applicability are illustrated here, we have decided to supplement our earlier expression of conclusions by this short article. In one respect, indeed, this has appeared to us as an obligation, since in certain par-

[1] This is one of a series of contributions by the Secondary Enrichment Investigation that are intended to present the results of specific portions of its work in advance of the complete memoir.

[2] ECON. GEOL., Vol. 12, No. 1, pp. 1–38, 1917.

[3] ECON. GEOL., Vol. 12, No. 4, pp. 379–386, 1917.

ticulars we now regard some of our former ideas as incorrect or unfounded.

Since the subjects here considered are to a large extent those with which our colleagues at Stanford have dealt, and thus are necessarily handled here in somewhat argumentative style, this paper would have been submitted to the Discussion columns of this Journal had it not been for the following circumstances. Before the appearance of Professor Tolman's discussion, my co-author, Dr. McLaughlin, left Cambridge and entered the Army. In consequence, any responsibility for expressions in this article must be borne by me alone. On the other hand, I know that in large measure Dr. McLaughlin and I hold similar views upon the topics discussed. Furthermore, it has seemed to me desirable, in order to define further our views on Engels, to record here, in much abridged form, certain of the ideas which he and I, with collaboration of other of our associates, have jointly accumulated upon the general subject of bornite and its alterations. Consequently these ideas cannot fairly appear under my name alone. Wherever I have felt sure of Dr. McLaughlin's views, pronouns of the first person appear in the plural, where I did not feel absolutely sure, I have adopted the singular form. In general, he may be jointly credited with anything that may be worth while in this article, but not held accountable for any of its shortcomings, since conference with him is now of course out of the question. Under these circumstances, I trust that the liberties I have taken in so unconventional a presentation of the paper may be accepted in the same tolerant spirit as other annoyances arising more or less directly from the War. Transmission has been held till now in order that, should any other discussions of our former paper have appeared, all might at a single sitting, as it were, be given recognition. I have profited much from discussion of the topics treated with my associate, Mr. D. A. Hall.

L. C. G.

PNEUMATOLYTIC OR MAGMATIC?—PNEUMOTECTIC.

Our application of the term *pneumatolytic* to what we regard as the earliest copper sulphides at Engels is criticized because the word is said to suggest vigorous destructive action, by such agencies, for example, as replace the rocks by tourmaline, topaz, scapolite or fluorite, whereas in reality, as we agree, tourmaline is unimportant at Engels and the other minerals are absent. We accept, of course, this connotation of the word pneumatolytic; indeed, we have regarded replacement by sulphides under the conditions we described as evidence of just such destructive action. For there seems to be no doubt in the minds of most students that sulphur is to be admitted on an equal footing among the other mineralizers, such as boron, chlorine, fluorine, and phosphorus. Yet we do not understand why sulphur should be

so included unless the development of sulphides under certain conditions may be regarded just as fully an effect of pneumatolysis as if the product were, say, tourmaline or topaz. And hitherto it appears not to have been considered requisite that two or more of these characteristic products should be present in order to prove pneumatolytic action—one only has sufficed, and very commonly one only is present. Then may not sulphides alone suffice, when they are clearly an early-stage, high-temperature corrosion product, and especially when, as in the Engels instance, they are accompanied in places by a little tourmaline to clinch the matter?

In advancing the term *pneumatolytic,* Bunsen employed it as if virtually restricted to the action of volcanic gases in and near fumaroles. Along with other gases, he emphasized the common effect of sulphur, or its volatile compounds, but made no mention of boron or fluorine. Obviously, any modern significance of the term is a liberal extension of the narrow sense in which Bunsen first used it. It is our feeling that we followed competent and predominant, even if not unanimous, authority in extending the term to such pegmatites and pegmatite-like knots, containing tourmaline and coarse crystals of titanite, as we found associated with sulphides at Engels, regardless of whether they were of destructive or constructive origin. If, in general, sharp-walled and dike-like bodies of rock called pegmatite, often causing no alteration of the enclosing walls, yet frequently containing notable quantities of such minerals as garnet, spodumene, titanite, topaz, fluorite, tourmaline and sulphides, and commonly characterized by coarse or erratic grain and by druses, as further indication of the influence or presence of volatile agents during crystallization—if such products are to be excluded from the pneumatolytic group, or if in this sense the term pneumatolytic may not be applied to ore deposits as well as to other mineral aggregates, then admittedly our understanding of the term was in error and our application of it to certain of the sulphide materials at Engels was indeed misleading. But even in that event, it seems to us that the difficulty is only shifted and not lessened

by giving to these earliest Engels sulphides the designation magmatic, as Professors Rogers and Tolman prefer to do.

As a matter of fact, since writing our Engels paper we have somewhat modified our views in regard to sulphide ores formed shortly subsequent to normal igneous-rock consolidation, and in consequence we realize that our reference to Sudbury may be open to criticism. In part this change of viewpoint arose from study of materials from a number of enlightening districts visited by Dr. McLaughlin in 1916; but in no small measure was it due to the array of testimony in the excellent article by Tolman and Rogers on the "Magmatic Sulphide Ores,"[4] which appeared while we were in the midst of that study, and which emphasizes so clearly the nearly or quite universal subsequency and animosity of the sulphides to the normal rock minerals. We sympathize with those authors' expressed lack of complete satisfaction with the term *magmatic* as applied to such sulphides but not with their compromise in retaining it. Although impressed by the importance of the part which gaseous mineralizers appear to have played in the localization and deposition of these sulphides, accomplished in part, at least, by corrosive replacement of already-formed rock constituents, we now feel, nevertheless, that the term *pneumatolytic* is not wholly satisfactory, for if used in its broader sense as outlined above, it does not apply with adequate specificness and exclusiveness to this group of immediately post-magmatic sulphides, nor, I am free to confess, does it perhaps sufficiently imply the close affiliations to the truly magmatic stage exhibited by the group as a whole; while on the other hand, if the term be limited to the narrower sense indicated at the beginning of the section, it applies of course still less satisfactorily.

Facing this dilemma in terminology, we concluded that there is need for two specific, definite names, each of which should be orthographically independent of the word *magmatic,* which has been applied to anything from a dry melt to cold water or carbon dioxide so long as there is reason to infer connection with igneous

[4] Leland Stanford Junior University Publications (1916).

rock processes. And in the hope of meeting this need, we concocted the adjectives: *orthotectic*,[5] to designate those processes and products, strictly magmatic in the narrowest sense, exemplified in the normal crystallization of normal igneous rocks, like granite or diorite, for instance; and *pneumotectic*[6] for those next-subsequent processes and products of magma consolidation in which fundamental influences of a sort that was magmatic in the strictest sense were recognizably modified and to some extent controlled by gaseous constituents or so-called mineralizers which accumulated in relative concentration, by elimination, when in the preceding, orthotectic stage the major portion of the magma crystallized without incorporating more than a minor proportion of those volatile substances that had been components of the initial molten solution.[6a] Thus, as we understand it, orthotectic probably does not signify solidification from dry fusion, although if there are natural melts devoid of water or other mineralizers, their crystallization products would fall in this group. Neither do the orthotectic products necessarily fail to include metalliferous ores, provided the ore minerals are on an identical footing with the customary igneous-rock minerals. But in case it is necessary to assume that the fluidity of the ore materials requisite for their localization is dependent on the influence of mineralizers, such deposits would fall in the pneumotectic group, which we regard as not necessarily implying, nor, on the other hand, necessarily excluding the action of gravity in any segregation or accumulation that may have taken place.

In consequence of the establishment of these two prior periods, the term *pneumatolytic* might be limited, probably with advantage, to those processes and products in which the fundamentally dominating influence is no longer the straight melt but is, in-

[5] From *orthos*, straight, strict, true, real, genuine, and *tēktos*, melted, molten.

[6] From *pneuma*, wind, air, spirit, exhalation—hence, gas.

[6a] Professor R. A. Daly has just suggested that in addition to gases yielded more or less locally in consequence of rock crystallization, gases of other derivations, *e. g.*, resurgent gases and gases migrated from other localities because of change in equilibrium conditions, may likewise exert a modifying influence on orthotectic control and thus yield pneumotectic products. In our present state of knowledge concerning magmatic processes, it seems advisable that the definitions given above be so expanded as to include this possibility.

stead, the still further concentrated and controlling gases. And on down the list one might go, with *pneumato-hydato-genetic* next—if it be deemed that there is a need for that intermediate term—on to the stages of *hydrothermal,* and beyond, if necessary or justifiable. Any and all of these periods or divisions would be collectively regarded as *magmatic* by those who believe in the versatile efficacy of igneous processes in mineral formation and deposition. By such a scheme, also, desirable qualifiers like late and early are left free for use within any of the periods, *e. g.,* the augite of a diabase is of late orthotectic age, that is, at least, as compared with the plagioclase laths. And it would seem as though such a group of terms might be found useful, and, what is more important, be used with identical meanings, by both petrographers and students of ore deposition.

In terms of such a classification, we would place the group of ore deposits mentioned on an earlier page in the pneumotectic class or period to express our convictions that these ores were formed under conditions closely allied to and but one step or stage removed from those of strictly magmatic character, yet in which gaseous agents, probably sulphur in particular, played a distinctly important part and caused the difference between the resulting product and the normal igneous rock (*e. g.,* norite or gabbro) with which such ores are associated. And in this light, coming to the specific case of those particular Engels sulphides which we formerly called pneumatolytic, we would now regard them as belonging in the pneumotectic group, though probably near its later margin, *i. e.,* on the side away from the strictly molten or orthotectic control. If this modification of view concerning a part of the Engels ore may be regarded as any approach toward the ideas held concerning it by Professors Rogers and Tolman, we are glad to be able to make it, as well as to acknowledge our indebtedness to them in aiding us to attain what we now regard as a truer understanding of the situation.

In contrast with the view that the Engels magnetite is all of one age and later than the normal rock-consolidation, our own evidence, I believe, does not justify us in insisting that two generations of the mineral are present. But it seems to me that a

reasonable interpretation of what the materials show favors the idea of two generations, unless all of the so-called "accessory" magnetite of igneous rocks is actually to be regarded as of late rather than early formation.

HYDROTHERMAL ORE.

Examination by each of us of all accessible parts of the Engels mine and subsequent study of the specimens collected have led us to the opinion that of much more quantitative and commercial importance in that district than the materials classed above as pneumotectic is the sulphide ore in which are plentifully contained hydrothermal minerals, chlorite, sericite, epidote, etc. Of this ore it has been argued that if the chlorite and sericite are actually later than the sulphide minerals, it would be necessary to assign these sulphides to a period earlier than our intense[7] hydrothermal, namely, to the magmatic or late magmatic of Rogers and Tolman or to our pneumatolytic (now pneumotectic). If it be granted that the chlorite and sericite are indeed later than the sulphides that surround them, we still feel unable to conclude that the two sets of minerals belong in different periods. On the contrary, the whole weight of evidence indicates to us that the chlorite and sericite and the sulphides are most intimately related as to period of formation. Just as we recognized time-difference or sequence as between chlorite, sericite and epidote within the same period, which we called intense hydrothermal, and just as in places we recognized a time-difference or sequence as between chalcopyrite and bornite—one locally corroding the other—without being able to entertain the idea that these two sulphides belong in different periods, so, also, we recognized a time-difference as between the sulphides and the associated

[7] We affixed the adjective *intense* in this case to cover that part or subdivision of the entire hydrothermal process which we believe was earlier and accomplished under conditions of greater temperature and pressure than that part which we designated as late hydrothermal. It might have been wiser, as we once considered, to call the first period *early hydrothermal* instead. For the actual degree or extent of alteration of the initial rock minerals is on the average considerably less and the change in outward appearance of the rocks decidedly less as a consequence of the early than of the later stages of hydrothermal alteration. The effects of the former, however, are more widespread and less localized than those of the latter.

gangue, yet felt and still feel that it would be unreasonable and impossible to put the two groups of minerals in separate periods. We cannot escape the conviction that if hydrothermal conditions governed while the chlorite, sericite, etc., were forming, they governed also while the sulphides were forming. But, as a matter of fact, notwithstanding additional arguments recently brought forward, we are still unable to accept the view that laths of chlorite or of sericite replace the Engels sulphides. If our former statement on this point may have implied that some doubt existed in our minds, the real deficiency was in our literary ability rather than in the matter of geological evidence. And we can hardly give assent to the implication that what Professor Kemp[8] has recently alluded to as "the natural conclusions which scores of careful petrographers have drawn from thousands of thin sections" are to be dismissed as "general and hazy notions founded on theory."

If the coincidence of direction of chlorite laths and bornite structure to which Professor Tolman refers was discovered in the Engels materials only after being photographed, one might infer that this was not a very noteworthy or common feature of the ore. This would accord with our experience, for neither in the Engels material nor in several hundred specimens containing bornite and sericite or chlorite, from numerous other districts, that have been studied by several workers in this laboratory has a single convincing illustration of this feature been found, although it has been deliberately sought for ever since it was first described. As against this, in the Engels specimens are literally hundreds of observed examples of chlorite-lath corrosion ranging from a mere "nibbling" of the edges to advanced attack and distortion such as we pictured. Although Professors Tolman and Rogers are convinced that such apparent corrosion may be present in gangue that has replaced the sulphides, I must confess inability to follow them. Their evidence which appeals to me most forcibly is that afforded by microphotographs of polished sections which show, traversing the sulphides, a gangue mineral in irregular, crack-following veinlets that extend away

[8] ECON. GEOL., Vol. 12, p. 634, 1917.

from, and as if continuous with, crystalline plates or laths of what is labelled or described as the same gangue mineral, and these laths have the corroded-looking outlines under discussion. Without having seen and studied these very specimens, no one may deny the descriptions given. But it may not be improper to mention instances of specimens studied in this laboratory that would afford the same kind of appearance as above noted when examined on the polished surface, but which, when carefully examined in thin section, have revealed that the irregular veinlets consist of a mineral different in kind and later in age than the one present as corroded laths. In the particular case of Engels, for example, irregular veinlets of carbonates and of epidote extend outward from corroded foils of chlorite or clear-cut plates of sericite, but the several gangue minerals cannot be safely identified or distinguished on the polished surface. That it would be natural for cracks to start or to continue from long thin slivers or plates of gangue which, interrupting the continuity of the sulphide, would cause weakness[9] there, is confirmed in many examples where the age relations are beyond question.

But in instances where the mineral identity of the veinlets and the corroded plates may be positively established there is even then a conceivable alternative for the conclusion that the plates, notwithstanding apparent corrosion, are, like the veinlets, later than the adjacent sulphides. For, as a former associate in field and laboratory, Mr. Y. S. Bonillas, was able to convince me fully at Bisbee, pyrite, earlier replacing country rock along irregular veinlets, was later overtaken by chalcopyrite which in places completely replaced the country rock that formed the walls of the pyrite veinlets but surrounded and retained the pyrite within itself, probably for the reason that, since both pyrite and chalcopyrite had but recently departed from the common source of the ore, they were reasonably well in equilibrium to each other, or the earlier reasonably in equilibrium to the solution from which the later was shortly afterward deposited. The appearance within the chalcopyrite portion certainly suggested that younger veinlets of pyrite cut through chalcopyrite. But when studied

[9] See pages 94–95 of this article.

in relation to the country rock, where this remained, the true state of affairs was made plain; and when such material was examined in polished section under the microscope it was found that the faceted margins of the pyrite grains were sufficiently corroded by the chalcopyrite to reveal both the faceting and the corrosion, but where the pyrite veinlets cut the country rock, no corrosion of the facets was found. Dr. Murdoch and I had examined some of this chalcopyrite-pyrite material from Bisbee several years before and reached the explanation mentioned, though we held and reported it with much uncertainty until Mr. Bonillas supplemented our evidence with the balance of the story, namely, what happened in the country rock where the chalcopyrite had not reached.

Translating this explanation to the case in hand, one might assume that veinlets and clusters of plates of a gangue mineral, like chlorite or tremolite, had formed by alteration processes in norite and then later sulphides came along and in places ate up all the remaining norite, but only partly replaced *the more nearly contemporaneous and consanguineous* prior alteration product, chlorite or tremolite; where this gangue mineral was well crystallized, corrosion effects would be evidenced by modification of the crystal outlines, but where existing in irregular veinlets, a considerable amount of corrosion might go on without sufficiently modifying the type of irregularity to be detected. I do not need to be reminded what liberties I am taking in even imagining that so complicated and unusual a hypothesis might be applied to material I have never seen and from districts most of which I have never visited; perhaps this may simply show the extent to which a conservative mind will grasp at straws before accepting, on the evidence as it now stands, an overturning of much of the long-established criteria of mineral sequence.

On the other hand, I can conceive that such corrosion-like features as are under discussion might result from practically simultaneous crystallization of the two constituents, so that the chlorite, starting bravely to form as well-shaped laths (in reality, plates or foils), was interfered with by the growing sulphide and thus prevented from fully and perfectly carrying out its design;

indeed such virtual contemporaneity might possibly explain those few instances of concordant orientation which Tolman and Rogers describe. We believe, as a matter of fact, that the sericite and the bornite close to it are virtually contemporaneous.

But, in general, if one is to repudiate the evidence of later corrosion of the chlorite by sulphides, then I am not clear as to how he may distinguish the corrosion of other silicates, such as the pyroxenes, which has been used so confidently as a proof that the sulphides were later. To our eyes, the corrosion in the two instances is similar, except as it may have been somewhat influenced by different structures and different chemical stabilities of the several kinds of silicate minerals attacked.

ORIGIN OF THE CHALCOCITE.

Since there is some reason to think that we gave rather confused expression to the ideas we intended to convey in regard to chalcocite and enrichment at Engels, I shall endeavor to state a part of the case more plainly and categorically, for I consider the subject as of much importance.

The ore at the Engels mine we believe to have undergone a relatively normal and comparatively uninterrupted history of downward enrichment, perhaps from the time the ore mass was first disclosed by erosion. The Engels chalcocite is a normal consequence of this normal enrichment history. Its dull, sooty character at the top, just under the zone of direct oxidation and leaching, as shown in specimens given me by Mr. Juessen and in remnants near the edges of the highest stopes, now caved, is a natural effect of oxidizing disintegration that is exhibited in many other regions. Such chalcocite, when examined under the microscope in polished section, is always more or less bluish in consequence of its content of dissolved covellite[10] which indicates that in the struggle between the oxidizing influences from the atmosphere and the reducing influences of the polysulphide minerals, those of oxidizing tendency are attaining the control.[11] Underneath this sooty material the chalcocite is more massive and

[10] Posnjak, Allen and Merwin, ECON. GEOL., Vol. 10, No. 6, p. 506, 1915.

[11] Since it is well known that oxidizing agents, like a ferric salt, for example, will form covellite from chalcocite.

steely, as is also common elsewhere. Most of this steely chalcocite lacks the blue caste of that occurring higher up, and only where the direct oxidizing influences are insinuating their way into the material is the bluish color to be seen. Lower still, bornite occurs increasingly with chalcocite; on the whole in the Engels mine as developed up to June, 1916, the proportion of chalcocite declines with depth and in the deepest portions it is increasingly confined to those lattice and graphic associations with the bornite which, from study in numerous other districts. I believe, are to be regarded, in the order stated, as the next-feeblest and the feeblest effects of chalcocite enrichment on that easiest of victims, bornite. At Engels, little of the blue chalcocite is to be seen in the specimens that contain bornite and white[12] chalcocite, for the probable reason that the zone of complete enrichment was there thick enough to separate most of the uppermost remaining bornite from that upper part of the chalcocite zone where the white chalcocite is being converted into blue by incipient oxidation. This also is verified elsewhere.

At the Superior mine, a somewhat different state of affairs exists. That ore deposit, we believe, underwent a normal enrichment history quite identical throughout most of its span with that at Engels. Chalcocite, bluish and sooty above, steely and white lower down, still lower associated with bornite, and at greater depth dwindling to only the graphic occurrence, was probably formed just as at Engels. But owing to difference in physiographic situation, the advent of erosion conditions which caused little modification of affairs at Engels worked havoc with most of the enrichment zone at Superior. For at a rate which under the new conditions outstripped that of descending oxidation and enrichment, so that these chemical effects were virtually at a standstill, erosion planed the deposit down so deep as to leave only the feeble bottom of the enrichment zone, only the lowermost or graphic phase of chalcocite. The present surface exposed at Superior we regard as the probable equivalent, from the

[12] What is here called white chalcocite may show minor color variations, with tinges of pink or blue, but on the whole is readily distinguishable from the decidedly bluish chalcocite to which this appellation is here applied.

standpoint of depth of enrichment, of a horizon at Engels somewhat below the present deepest workings.[13] The slight amount of oxidation that has been able to go on from this rapidly eroding surface at Superior has liberated the small quantity of copper now represented in sparing, minute veinlets of chalcocite, which constitute a superimposition of the beginnings of a modern enrichment upon the stumps of the earlier, interrupted enrichment. The chalcocite of these tiny veinlets is of the bluish kind, and this is exactly what one would expect, and what one finds elsewhere, when enrichment, taking place so close to the surface as is the case in the Superior exposures, forces upon the chalcocite, in the form of covellite content, evidence of the domination of oxidizing influences close by. Thus the minute bluish chalcocite veinlets at the Superior mine and the dull, sooty, bluish (when polished) chalcocite at the top of the Engels ore body owe their bluish color largely to the same cause and they are in a sense of analogous import, though at Superior the chalcocite is blue from the beginning, as it forms from bornite, while at Engels the blue chalcocite is being derived from what was white chalcocite. Under such circumstances, I may remark, there is reason to think that the blue color may persist deeper at Superior than it does at Engels.

The relation between the chalcocite of the rims that border grains of bornite and the chalcocite of the graphic structures in bornite raises a large question on which much might be said, though to be effective it should be presented in connection with matters not immediately concerned with the title of this particular discussion. Perhaps, therefore, I may be excused for recording here somewhat dogmatically those of our ideas which

[13] In both mines, at some still greater depth, if the deposits persist, I expect that chalcocite may cease entirely. What that depth may be I am unable to predict, though from indications afforded by somewhat analogous conditions elsewhere I am prepared to believe that it may be considerable, perhaps several hundred feet at least. Possibly if enrichment at Engels had not continued after and while enrichment at Superior was suspended, the deepest ore now exposed in the Engels mine might show just about the same degree of enrichment as that now revealed near the surface at Superior instead of showing, as it actually does, a somewhat more advanced degree.

bear directly on this question as it pertains to the deposits at Engels.

Although we regard the Engels mine chalcocite as the product of an enrichment history in which no important breaks are to be detected, this does not mean that we regard all the chalcocite as of exactly the same age. There is growing evidence to indicate that enrichment is a comparatively slow process. In the Engels mine I fancy that chalcocite is, or recently was, forming at any or all elevations from the uppermost bornite remnants to the deepest reaches to which chalcocite extends, and some of this recent chalcocite of the upper levels has formed next to, or close to, chalcocite which, earlier in the enrichment period, was at the bottom of the then-existing enrichment zone. Specifically, I believe the marginal rims of chalcocite are on the whole younger than the chalcocite of the graphic structures and represent the downward encroachment of somewhat more vigorous enriching conditions on the feebler type that had formed nearer what was then the bottom of the enrichment zone. I believe a distinct decrease in the proportion of the rimming chalcocite is to be detected with depth at Engels, and at the Superior mine, regarded as a still deeper equivalent, rims of *white* chalcocite are practically, perhaps entirely, wanting.

The striking dependence on gangue boundaries shown by the chalcocite rims is, so far as can be ascertained, the consequence of physical conditions only—there is apparently, as between gangue minerals and sulphides, a sufficiently wide difference in such properties as elasticity, compressibility, and temperature-volume ratio as under certain conditions to convert the plane of contact between the two into a place of weakness, a fissure, and therefore an easy channelway for solutions. Consequently, altering solutions perform their attack especially along such avenues. There is thus no fundamental genetic connection between the chalcocite and the gangue minerals along whose boundaries it forms. That the rims are especially common around sericite laths does not mean that the sericite and the chalcocite are related in origin but only that the sericite, because of its particular phys-

ical properties, exaggerates more than do other gangue minerals the weakness-surface about its boundaries, and in this effect it may be aided by permeability along its facile cleavages. This sort of thing, abundantly illustrated in other districts, is not confined to gangue boundaries but may take place about the margins of harder metalliferous minerals, like magnetite and pyrite, where these are embedded in the softer bornite, and for precisely the same reason—viz., difference in physical properties. There is, I believe, evidence to suggest that in an ore just formed or in process of formation, these surfaces of weakness between unlike minerals are only potential or are at best but slightly developed, and that generally it is only after the lapse of time that stresses arising from such causes as crustal adjustments large or small, change in depth of burial, the vicissitudes of temperature, and chemical-volume changes convert these potential places of weakness into actual rupture than then allows the relatively easy ingress of solutions. If this is so, it would follow that any mineral introduced along such channelways would, *a priori,* be suspected of belonging to an essentially later epoch than those on either side of it, and if this introduced mineral were chalcocite replacing bornite—a combination so particularly characteristic of downward enrichment—there would seem to be afforded strong presumptive evidence that the chalcocite was indeed due to that very process.

There are, furthermore, growing indications that a relatively vigorous and capable copper-sulphate solution, in the early stages of its attack upon bornite at any particular locus, closely confines its enrichment effect to the walls of the more easy, though commonly only minute, channelways such as are afforded by the numerous microscopic cracks and these gangue-margin planes of weakness. From these avenues, the chalcocite is gradually widened until finally, if the process continues sufficiently long, all the bornite may be converted to chalcocite. The borders of chalcocite between bornite and gangue shown so strikingly in much of the Engels material represent a not far advanced stage of such a mechanism.

When, however, the copper-sulphate solution has migrated almost to the far limits or bottom of the enrichment zone, it will have lost part of the vigor and capacity for enrichment with which it started out and with which it operated at higher levels. This decline in capability results in part from dissipation of its effective reagent, copper sulphate, by circulation or diffusion to places out of reach of enrichable sulphides, in part from exhaustion of the copper sulphate in consequence of the enriching work it has done higher up, in part, perhaps, from concomitant accumulation in the solution of inert (from the particular standpoint of chalcocite formation) solutes, like iron sulphate and sulphuric acid, and in large part, no doubt, from dilution due to mingling with and incorporation of neutral, inert or foreign waters, as well as wholly exhausted solutions that were its predecessors.

There is reason for thinking that when such relatively enfeebled solutions come to attack bornite, they are no longer subject to crack and gangue-boundary control but are governed by a different and more delicate kind of influence. I believe we are on the trail of what may be this influence—in a word, variations in the bornite itself—and when some further tests of the hypothesis have been made, we may feel justified in presenting the evidence in some completeness. At any rate, the abandonment by the chalcocite of the fracture channelways and its formation at places determined by different influences may result in a puzzling distribution of the chalcocite and may produce, I believe, those peculiar patterns known as the lattice structure and the graphic structure, the last-named being, in my regard, the final, expiring accomplishment of any given portion of enfeebled enriching solution.

In the normal downward descent of the enrichment zone as a whole, solutions of intermediate vigor, still controlled principally by the gangue-boundary channelways, should reach the horizon which, sometime earlier in the period of enrichment, was the lowest part of the zone and thus the scene of operations of the enfeebled solutions just discussed. Under such circumstances, a composite result should be produced: graphic shapes

of the earlier chalcocite would be found left in the central portions of a bornite field whose marginal portions had been converted into the later chalcocite rims by the somewhat more active and younger solutions; and from these marginal rims there might extend a little way into the central bornite groups of more or less parallel tongues and slivers arranged in two or three directions, thus exhibiting a sort of indecisive balance between gangue-boundary control and the next more feeble, lattice-structure control, in other words the beginnings of a transition from the one to the other; and in occasional blebs[14] of graphic chalcocite there might be expected residues of bornite in nearly obliterated lattice arrangement, as if the graphic chalcocite might in part or in places have passed through the lattice stage, thus affording evidence of another transition, this time from the next feeblest to the feeblest type of enrichment.

All these relations are actually to be seen in the Engels material, from the intermediate and deeper explored horizons, that shows central graphics and marginal rims of chalcocite in and about bornite areas, and they are, I believe, capable of explanation by the hypothesis just outlined. Rogers's uncertainty regarding the questions whether there are one or two "generations" of chalcocite present and whether one or both or neither is of descending origin may thus possibly be cleared up. Still higher up at Engels, the lattice and to some extent the graphic structures are obviously being obliterated as a result of the further enrichment caused by solutions still more vigorous in character and consequently less subject to the influence of any of the controlling factors already mentioned. Perhaps the strong solutions would have been able to accomplish at a given place all the noted phases of chalcocite replacement had not all the easiest types of enrichment been already accomplished by the weaker solutions when they first reached that place earlier in the enrichment period.

The contrast between the feeble and the vigorous types of en-

[14] This term may be used for want of a better to designate the peculiar irregular small individual units of either mineral that, collectively, comprise the graphic pattern or structure.

richment is strikingly shown in the Superior material, the erratically distributed graphic chalcocite representing the former and the latter being manifested by the narrow blue chalcocite veinlets which, in contrast, follow definite cracks in the bornite or hug the margins of gangue and magnetite grains. Possibly the greater prevalence of cracks cutting through the middle portions of bornite areas at Superior than at Engels may be a consequence of the extreme fluctuations of temperature to which the ore so close to the surface has been exposed at the former mine. The conditions at Superior further afford occasion to contrast the difference between *extent* or *degree* of enrichment and *character* or *vigor* of enrichment. For although the graphic chalcocite at Superior accounts for considerably more copper and thus indicates a more advanced degree of enrichment, quantitatively, than the chalcocite of the blue veinlets, the former is about the weakest kind of enrichment possible, while the latter is about the most vigorous kind possible. The one is feeble because the process at that place and time was approaching exhaustion; the other is scanty only because its vigorous causes have been allowed by rapid degradation but a comparatively brief time in which to work. It is to be emphasized again that this bringing into juxtaposition of a vigorous and a feeble kind of enrichment as at Superior is not a normal consequence of the orderly progress of the enrichment process, but on the contrary implies a cessation and then a renewal of enrichment, with intervening degradation, so that the later, more active, shallower-seated enrichment is superimposed upon the earlier, weaker, deeper-formed kind. It is with such an explanation that we believe the physiographic features at Superior and of the surrounding region are in accord.

I suppose it will not be possible to prove conclusively that all the chalcocite at the Engels and Superior mines is of downward-enrichment origin until, if ever, mining operations shall have descended to a depth where, though bornite continues, chalcocite has entirely given out. But for my own part, in the light of my present conceptions of, and acquaintance with, bornite enrichment, I believe the chalcocite is all secondary, in the good old-

fashioned, downward sense. The only alternative I could reasonably entertain is that a part of the graphic chalcocite may be due to simultaneous intergrowth with the bornite and is thus primary in every sense of the word. I know that Dr. McLaughlin is inclined to ascribe more weight to this possibility than I do, though I recognize it as entertainable, for I am more than ever of the opinion that graphic structures may be formed both by replacement and by contemporaneous intergrowth.

REPLACEMENT IN THE BENDIGO QUARTZ VEINS AND ITS RELATION TO GOLD DEPOSITION.*

F. L. Stillwell, D.Sc.

INTRODUCTION.

The geology of the Bendigo Goldfield has been well known since the publication of the Special Reports on Bendigo by E. J. Dunn in 1893 and 1896 by the Mines Department of Victoria. During the same period two reports by T. A. Rickard were published in America[1] which drew further attention to the peculiarities of the saddle reefs. The location of these peculiar reefs along the axes of the anticlines of folded sediments is a feature of these reports, but the same fact had, however, previously been described in mining literature by W. Nicholas,[2] whose name has been omitted from the publications of Dunn and Rickard. The saddle reefs had also been represented on geological survey plans of some of the mines on the Garden Gully line of reef by Caleb Thomas in 1879, and the work of these earlier observers is remarkably accurate and reliable. These reports together with a later series prepared by H. S. Whitelaw, of the Victorian Geological Survey, and a compilation of the mining work in the central portion of the field by V. Pabst,[3] present a mass of information to the investigator into the factors influencing the distribution of gold in the quartz reefs.

* An investigation into the causes affecting the distribution of gold in quartz was initiated by the Advisory Council of Science and Industry of Australia in July, 1916. The author was appointed to devote his whole time to the question and Bulletin No. 4 has recently been published containing the results of the first year's work.

[1] T. A. Rickard, "The Bendigo Goldfield," *Trans. Amer. Inst. Min. Engr.*, 1892; "Origin of Gold-bearing Quartz of Bendigo Reefs," *ibid.*, 1893, p. 289.

[2] W. Nicholas, "Golden Quartz Reefs of Australia," *Min. Journ.*, London, 1884.

[3] V. Pabst, "Resources of the Central Area, Bendigo Goldfield," McCarron, Bird & Co., Melbourne, 1917.

GENERAL GEOLOGY OF BENDIGO.

The Bendigo Goldfield is situated 100 miles NNW. of Melbourne in an area of Lower Ordovician slates and sandstones. The rocks are folded into a number of well-formed anticlines and synclines, which, at times, are found to roll out and be absorbed by the neighboring fold. There are no well-marked horizons by which the same bed may be recognized across neighboring anticlines in a transverse direction. In the direction of the strike the anticlines pitch, in general, to the north and south from the center of the field.

A large mass of granodiorite outcrops at the surface nine miles south of Bendigo. It is intrusive into the Ordovician sediments and is considered to be Devonian in age. Its presence is generally believed to be genetically connected with the existence of the quartz reefs.

A system of parallel monchiquite dykes intersects the reefs and strata and follows closely the trend of the anticlinal axes. The different dykes only show very minor variations and are probably all of Middle Tertiary age.

The presence of numerous faults has been proved by the mining operations. Strike faults are by far the most abundant and a large number of these have a lateral displacement of a few inches up to two or three feet; but there are a number of stronger faults which have displaced center country for 50′ or 100′. Oblique faults, including the cross-courses, are not so abundant. The great majority of the faults are reverse or upthrust faults, and it may be pointed out that, if the faulting belongs to the period of folding, then reversed faults must necessarily occur. During folding a base line is tremendously foreshortened, and only reversed faults are compatible with such foreshortening. In no instance had a monchiquite dyke been observed by me to be cut and displaced by a fault. In this respect the recent work is at variance with Dunn's observations, but shows agreement with the earlier work of Nicholas.

THE QUARTZ REEFS.

The quartz reefs are grouped according to their form into

1. Bedded reefs (including saddle, trough and leg reefs).
2. Fault reefs.
3. Spurs.

The saddle reefs of Bendigo are the most widely known. The saddles often, but not always, form immediately under a strike fault as it crosses center country. Frequently bodies of quartz extend upwards along the fault plane, forming the neck of the reef. Trough reefs are related to the synclines as the saddle reefs are to the anticlines; but they are not so important and are usually of smaller dimensions. The leg reefs are the downward extensions of the legs of the saddle reefs. They are usually very small but have been very productive in some instances. The fault reefs occur along the fault planes but have only been successfully worked in isolated instances. Much more important are the spurs, and a large amount of gold has been won from massive spurry deposits of quartz. These reefs usually "make" in the footwall from the "back" of a bedded reef.

The reefs often contain numerous blocks of slate and sandstone. Frequently there are films of black slaty material which, especially in the leg reefs, possess a more or less parallel appearance producing the laminated quartz. These black films consist largely of carbonaceous matter.

The mineral content is variable and small in comparison with the amount of quartz. The variable distribution of the gold is the pressing economic problem. The associated sulphides, arsenopyrite, pyrite, pyrrhotite, blende and galena, are also variable, but do not always show the same variation as the gold. The nonmetallic minerals, ankerite, calcite, chlorite, white mica and felspar occur in the reefs. In some cases the ankerite forms a lining ½ to ¼ inch thick along the walls of spurs, in a manner similar to crustification. Such encrustations may follow all the corners of angular fragments of country rock enclosed in saddle and trough reefs. The ankerite is often seen in this manner in the neighborhood of gold shoots. Chlorite and white mica are wide-

spread through the reefs, and the chlorite always betrays its presence by coloring the quartz green. The felspar is less common.

Since the dykes are now looked upon as of one age, and since the dykes everywhere cut the reefs it is certain that the large majority of the reefs are older than the dykes. Only two instances have been recorded where small 2-inch spurs have intersected a dyke. Further, since it is believed that the quartz reefs and the granodiorite are inter-related it becomes probable that the formation of the reefs is confined to one metallo-genetic epoch.

FORMATION OF THE QUARTZ REEFS.

The quartz reefs are looked upon as deposited from ascending aqueous solutions, and the conception of the solutions as forcibly intrusive, in the same manner as igneous dykes, is very helpful in picturing their origin.[4] Hitherto the formation of the saddle reefs has been pictured as due to the accumulation of the mineral solutions in cavities formed during the arching of the strata. The form of the saddle and trough reefs, the common brecciated structure, and the presence of the small radial spurs that enter the footwall of the saddle is strong evidence of fissuring. The ankerite lining that has formed around some of these inclusions and on the walls of some of the reefs seems to be evidence of the filling of fissures. But it is now known that a portion of the reefs has been formed by the replacement of country rock by quartz.

REPLACEMENT.

Laminated quartz is very common and frequently carries the most gold. Its origin is therefore very important.

Dunn[5] considers that the thin laminæ are films of slate defilling of fissures. But it is now known that a portion of the reefs The quartz between the films is supposed to grow wider, until, in some of the larger reefs, the distances between the laminæ are feet wide. This explanation involving successive openings and

[4] Blamey Stevens, "Intrusive Pressure of Mineralizing Solutions," *Min. Mag.*, Vol. XI., 1914, p. 313.

[5] E. J. Dunn, "Report on the Bendigo Goldfield," p. 24.

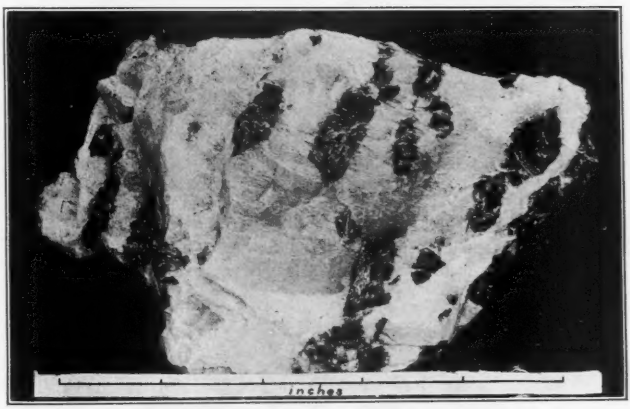
successive scalings of the wall rock is confronted by grave difficulties and was combated in 1881 by Nicholas[6] in an article advocating the growth of quartz reefs by the "transmutation" of slate and sandstone beds. The wavy, tortuous course pursued by some of the laminæ, the appearance of groups of laminæ with frayed-out ends, the reappearance of such groups across areas of white quartz, the association of gold and minerals on both sides of the laminæ, and the microscopic characters of the laminæ are unexplained.

Lindgren[7] has looked upon the banded structure in quartz veins as the result of the filling of several closely spaced fissures; but this explanation does not suit the majority of instances.

Sometimes the quartz has the appearance (Plate V.) of magpie quartz and the first examination suggests that broken fragments of country rock are set in a quartz matrix. In a transverse direction of the same specimen the fine ragged ends of the dark laminæ can be distinguished. Examined in thin section the specimen does not contain any slate that can be identified as such. The darker fragments are only marked by the presence of minute opaque inclusions in the quartz. The sharp boundaries in the hand specimen are equally marked under the microscope by a regular edge of the aggregated inclusions (Plate VI., *A*) which bear no relation to the boundaries of the quartz crystals, and which may cut indiscriminately across two crystals. The opaque inclusions in the darkened area may be evenly distributed through the quartz, but usually one can recognize the trend of parallel lines in them, which may indicate the original slaty cleavage of the replaced fragment. There are all stages between the sets of evenly distributed inclusions and sets in which there is a partial aggregation of the inclusions into clots and lines. In some cases of partial aggregation of the inclusions the boundary of the former slate fragment is no longer marked by the edge of the inclusions, but by a very minute difference in the positions of extinction of

[6] W. Nicholas, "Origin and Growth of Gold and Quartz Reefs in the Lower Silurian Rocks," read before the Roy. Soc. Vic., Aug. 11, 1881, printed in the *Bendigo Advertiser*, Aug. 13 and 20, 1881.

[7] Lindgren, "Mineral Deposits," p. 154.



Preserved brecciation in a specimen of quartz from 2122-ft. level, Sea Shaft, Bendigo.

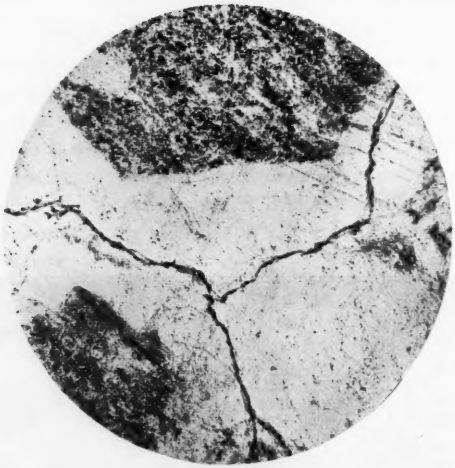


FIG. *A*.

Thin section of specimen illustrated in Plate V. Group of carbonaceous inclusions embedded in quartz, × 46.

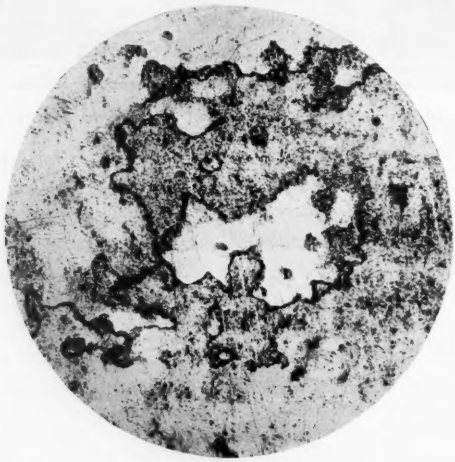


FIG. *B*.

Another field of Plate V. A crystal of clear quartz with irregular boundary developed within an area of carbonaceous inclusion, × 46.

the quartz crystal on either side of the former boundary. The final stage is indicated (Plate VI., *B*) where a crystal of quartz, absolutely clear and free from inclusions, has developed in the middle of an area of opaque inclusions. The boundary of this clear crystal is very irregular and marked by a dark line, which undoubtedly represents an aggregation of the opaque particles formerly resident in the quartz which is now clear. The inference is clear that the black fragmental areas in the specimen formerly represented fragments of slate, which have been replaced by quartz. The slate constituents, including mica, have been removed, except for the opaque matter which is an insoluble carbonaceous residue. A final stage of the replacement process results in throwing this residue out of the crystal where it forms the dark laminæ.

Another thin section cut at right angles to the preceding passes through an area of dark laminæ. The section consists of quartz, seamed with a series of dark lines which are thickened in places by ankerite. As a rule the lines are evident in ordinary light by their opacity; but in polarized light they are marked by lines of finely granular quartz. This feature is a normal characteristic of the laminæ under most conditions, and it seems necessary to associate the growth of the dark laminæ with the formation of this finely granular quartz.

In other instances the dark laminæ are rendered visible between crossed nicols by minute flakes of brightly polarizing mica, which are not easily distinguished from the quartz in ordinary light. This indicates in another way the connection between the laminæ and slate; and the relation is confirmed by the very common transition on a microscopic scale, in hand specimens, and in the reefs themselves, of thin laminæ into normal fragments of slate.

The brecciated character of the magpie quartz indicates that fissuring or shattering occurred prior to the deposition and replacement of the ore. In many instances, however, in the larger reefs the included blocks of country rock are not replaced in the slightest degree.

CHLORITIC QUARTZ.

The recognition of the dark laminæ as residues left after the replacement of slate by quartz leads to the explanation of the presence of chlorite and white mica. In thin section the pale green chlorite is embedded in the quartz and assumes a peculiar, fibrous, segmental aggregate. The fibers are always developed parallel to the radius of the segment. When the green color deepens the number of these aggregates increases, and the proportion of the quartz correspondingly decreases until a massive aggregate of chlorite results.

In some instances fibers of white mica are interlaced with the fibers of chlorite in these aggregates. The white mica is similar to the white mica that may be seen in neighboring slate fragments. In other cases wedges of ankerite are observed between the fibers of chlorite, and the proportion of ankerite may increase until the whole of the chlorite is replaced by ankerite.

There is therefore clear evidence for the alteration from white mica into chlorite, and from chlorite into ankerite. As the white mica is removed from the slate by the mineralizing solutions it may be transported and embedded in the quartz reefs in any of these forms.

RELATION OF FAULTING TO THE REEFS.

The rock-folding, faulting, and reef formation are related phenomena. In numerous instances saddle reefs have formed underneath strike faults as they cross center country. Similar faults may intersect and apparently displace them. Variation in the size and the content of the reef is exceedingly common in the neighborhood of the intersection of a fault.

A large number of faults contain thin threads of quartz. Other minerals are often seen and the gouge of the fault plane has been proved by assay to be auriferous in every case examined. It is possible that the gold content of the gouge has been derived from the gold-bearing reef itself. Yet the existence of payable fault reefs in isolated instances, and the occurrence of a trace of gold in all instances, combined with the rare observation of any-

thing but primary gold in the reefs, indicate that the gold in the fault planes is probably primary, and deposited there in the first instance by the mineralizing solutions. The fault planes are thus likely to be the connecting channels for the mineralizing solutions between the isolated reef channels.

When reefs are displaced by faults it has always been assumed by Dunn and others that the reef is older than the fault. The same fault which displaces a leg reef may form a saddle reef in center country (Fig. 24) and the saddle reefs, on this assumption,

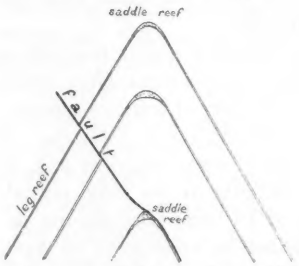


FIG. 24. Sketch of a fault which displaces the leg reef and "makes" a saddle reef in center country.

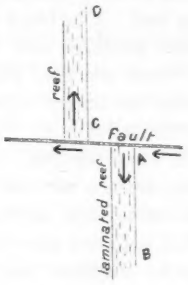


FIG. 25. Sketch of the intersection of a fault and leg reef.

have to be regarded as possessing differing ages. If, however, the fault planes are the connecting channels the reef is not necessarily older than the fault, as the leg reefs are in the main replacement deposits. Suppose a bed *AB* (Fig. 25) has been displaced to *CD*, and at the same time slipping and shattering occurs along the bed *ABCD* as the result of the folding movements. If the conditions are suitable for the replacement of the bed *ABCD* the mineralizing solutions rise along the fault plane and flow downward along *AB* and upward along *CD*, and the slate bed is replaced by quartz. In such a case the reef is younger than the fault; and the beds only, not the reef, have been displaced by the fault.

With this point of view there is no evidence to show that the

main quartz deposition continued through several geological periods, and that the amount of younger quartz is important.

OCCURRENCE OF GOLD.

There are examples in the history of Bendigo where saddle reefs have been productive for a length of about a mile and a half, but usually the gold shoots in the quartz reefs are very limited. Sometimes the variation in gold content is extremely rapid and an illustration has recently been provided by the results of two adjoining 50-foot tribute blocks on an east-dipping leg reef. One block yielded 1,157 oz. from 182 loads, while the other produced only 1 oz. from 16 loads.

When the gold shoots occur in laminated quartz the gold is often intimately mixed with opaque carbonaceous films which form the laminæ. The gold may form part of intricate mechanical mixtures with arsenopyrite, pyrite, galena and blende. It may form a nucleus for the crystals of these sulphides and, at the same time, be found on the outskirts of the same crystal. This indicates that the deposition of all these minerals has occurred together and that one cannot be considered to have caused the deposition of the other.

The intimate mixtures of gold or sulphides with the carbonaceous matter has a different meaning. This material existed in the slates before the arrival of the mineral solutions since it is believed that it constitutes the insoluble residue of preexisting slates. It can, therefore, be looked upon as a precipitating agency. Even when the gold is found embedded in ankerite or mica there are frequently opaque particles attached to it. Significant in the same way is the occurrence of gold in the slaty gouge and reef casing on the walls of rich reefs.

When the gold-bearing spurs are partially lined with ankerite the gold and the sulphides are usually situated along the junction of the quartz and the ankerite.

FACTORS INFLUENCING THE DEPOSITION OF GOLD.

Various factors have previously been regarded as favorable to the occurrence of gold. At times certain bands of rock have

been favored. As, however, most of the reefs are bedded it is clear that the limitation in length of the gold shoot is quite independent of the wall rock.

Certain of the slates can precipitate gold, and spurs which cross both slate and sandstone have in certain cases been notably richer in the slates than in the sandstones. In this respect certain slates are relatively favorable, but the fact that the walls of rich spurs are frequently lined with ankerite is evidence in favor of the opposing view that the character of the wall rock has little significance.

A stratigraphical zone theory has been advanced by Dunn and implies that rocks of a certain horizon contain the most productive reefs; but it does not explain the distribution of gold in any individual reef. Another zone theory states that the payable portions of the reefs are to be found in those portions of channels intersected by diagonal vertical zones; but it fails to explain the cases where one reef is profitable and the one immediately below it is unprofitable.

The neighborhood of the intersection of a fault and the reef has always been a favored region and many instances might be quoted where a reef has become productive in such circumstances. On the other hand similar intersections have been opened up without profitable discoveries. Cross spurs also have sometimes been credited as the cause of such a change in a reef.

The neighborhood of the intersection of a dyke and a reef was, at one time, a very favored locality, but since there is now evidence to show that the dykes are geologically much younger than the reefs, the only action they can have is secondary, through the agency of solutions traversing the walls of the dyke channel. There can only be a solution and a re-precipitation of the primary gold in the intersected reefs. Fragments containing gold have been observed embedded in the dyke and paint gold has been observed on the walls of the dyke. Gold has also been observed in the minute fissures in the reef quartz, caused by the dyke intrusion, within an inch of the dyke wall. In the last two cases it is clearly secondary gold, and such action is highly local.

A knowledge of the replacement origin of the laminated quartz seems to throw some light on the distribution of the gold, providing it is assumed that the fault planes are the inlet channels of the mineral solutions. It must be recognized that the solutions must have been very dilute in gold and that a rich shoot of gold must represent a natural gold concentrate, and further that the concentrating factor is most likely a chemical one. The irregular and variable character of the gold shoots seems to preclude the supposition of a simple variation in the gold content of the mineral solutions.

The solutions, according to this view, percolate from the fault fissures into the reef channel. The slate in the immediate neighborhood of the inlet at *A* (Fig. 25) is first replaced and the carbonaceous residues formed. The newly formed reef around *A* is in reality an irregularly porous mass and does not completely block the flow of the solutions. The carbonaceous residues are thrown out to the boundaries of the newly formed quartz crystals and portions are therefore likely to lie along some of the irregularly porous channels, along which the solutions are still passing. Along such channels the gold in the passing solutions will be precipitated by the carbonaceous precipitant. The earlier replaced zone around the place of entry can thus act as a precipitating barrier to, or a chemical filter of, the gold content of the solutions entering the reef channel.

The other sulphides, galena, blende, arsenopyrite, pyrite and pyrrhotite are likely to be thrown out in a similar manner. But for each mineral there will be varying combinations of temperature and pressure, amounts of dissolved gases and other causes which will cause variation in the deposition of each mineral.

The gold shoot, on this hypothesis, will not necessarily appear immediately at the intersection of the fault and reef channel, because the irregular channels through the earlier replaced zone at *A* may not contain sufficient precipitant precisely at the point of intersection. Nor will the gold shoot be evenly distributed on both sides of the intersection because the flow of solution into the shattered zone will probably be unequal in the opposite directions.

Along the fault plane there will be an irregular flow of the solutions. There will be places of maximum flow and places where the flow is almost nil. This variation of flow suggests an explanation of the intermittent nature of gold shoots along the intersection of fault and reef.

The hypothesis does not indicate that all intersections of faults and reefs should provide gold shoots. It is conceivable that the mineral solutions might issue from a fault plane into a reef channel, along the reef channel into another fault plane, then into another reef channel and so on. A gold shoot would not necessarily form in this manner at an intersection where the solutions had issued from the reef channel into the fault plane. If all the gold were precipitated near the first place of entry into a replacement zone, and if the flow of solution continued through another fault into a second reef channel no gold shoot could form at the second place of entry.

In the case of gold shoots in saddle reefs and spurry reefs the circumstances are probably not quite the same as those pictured in the leg reefs. The average reef of this type is much larger than the average leg reef. The fissuring has probably been more important in these reef channels and consequently a much greater volume of solution has passed along them. If in the course of the lateral flow along the reef channel the solutions pass into a zone of replacement a gold shoot would form in the neighborhood of the change.

Observations tend to show that the slates have been much more extensively replaced than the sandstones. This would be easily understood if the uprising solutions were saturated in silica. If this is so, a reason is provided why certain beds have been looked upon as favorable to gold deposition.

A TUNGSTEN DEPOSIT NEAR FAIRBANKS, ALASKA.

ALAN M. BATEMAN.

Industrial conditions imposed by the war have created a greater demand for tungsten than at any time since the metal first became of commercial importance. Its profitable occurrences are few, and the size of the deposits small, otherwise its value would not be as great as it is, and deposits that could not be worked before the war are being more or less vigorously exploited today. Our knowledge of the occurrence of tungsten may still be termed meager, and any additional information is at present of value, even though it is purely descriptive.

Tungsten minerals have been found for a long time in the sluice boxes of the gold placer workings of the Fairbanks district, but only recently have lode deposits been discovered. The following is a brief description of a tungsten occurrence near Fairbanks, Alaska.

GENERAL.

The property has been known as the Stepovitch tungsten property and is located in the Fairbanks mining district, about eighteen miles east of the town of Fairbanks, Alaska. It lies on the divide between the headwaters of Gilmour and Fish creeks at an elevation of about 2,450 feet and is six miles distant from a railway and automobile road. The property is as yet a prospect, although one shipment of 265 tons of ore has been made. It has been worked intermittently in a haphazard manner. The workings consist of an inclined shaft to a vertical depth of fifty feet and one cross-cut, twenty-five feet in length. The ore shipped was removed from a small stope, halfway down the shaft.

The climatic conditions at the property are similar to those of the Fairbanks district in general, with fairly long warm summers, but extremely cold winters. The precipitation is less than twelve inches a year, so that the small run-off is a serious handi-

cap in mining operations. The snowfall is not sufficient to inconvenience transportation of supplies or ore.

PHYSICAL FEATURES.

Topographically the Fairbanks district seems out of place in the northern latitudes. To reach it one traverses hundreds of miles of country exhibiting the most accentuated forms of glacial topography, but the Fairbanks district itself is in sharp contrast, for it exhibits no glacial forms and never has been glaciated. It is a lowland country of moderate relief with flat or slightly rounded ridges, rising to elevations mostly less than 2,500 feet above sea level. These ridges are monotonously repeated throughout the region, but are relieved here and there by broad dome-shaped masses such as Ester Dome and Pedro Dome. The main streams flow in flat valleys which terminate fairly abruptly against hillsides of moderate grade. Innumerable tributary creeks dissect the country and flow in steep narrow valleys about 1,000 feet beneath the hill tops. The valleys are filled with alluvium and well timbered. The hills are mostly mantled by a covering of soil derived from the decay of the immediately underlying rocks. The hill slopes and ridges are covered by small timber up to an elevation of about 2,400 feet. Rock outcrops are not numerous.

Most of the Fairbanks district is underlain by the Birch Creek schists, a much metamorphosed series, chiefly of sedimentary origin[1] consisting of quartzites, quartz schists, mica schists, hornblende schists, crystalline limestone and other varieties of metamorphic rocks. In places the bedding is readily discernible. These rocks are intruded by masses and dikes of quartz diorite and granite porphyry.

The Stepovitch tungsten property occurs in the Birch Creek schists in the immediate vicinity of an intrusion of porphyritic granite. In the hand specimen the granite is seen to consist of phenocrysts of feldspar embedded in a ground-mass of quartz, feldspar, and biotite. The microscope shows the rock to be made up of microcline, orthoclase, soda-lime feldspar, quartz, biotite, muscovite, hornblende, zircon, and titanite.

[1] Prindle, L. M., and Katz, F. J., Bull. U. S. Geol. Surv. No. 525, p. 59, 1913.

ORE DEPOSIT.

The ore is valuable for its tungsten content, which is entirely in the form of scheelite. No metallic minerals whatever could be observed in the ore with a hand lens; panning tests yield a concentrate of almost pure scheelite and show that no other high specific gravity minerals are present. The gangue is the enclosing rock.

The deposit occurs within the Birch Creek schists, which, in this locality, consist of crystalline limestone and mica schists with well-pronounced bedding dipping at an angle of 35 degrees. The crystalline limestone at a distance of about 100 feet from the deposit is a grayish, sugary marble, and appears to be fairly pure. The mica schist is made up chiefly of quartz, biotite, sericite, feldspar, and chlorite.

The ore appears to be a replacement and impregnation of the limestone along its contact with the mica schist. The crystalline limestone is highly altered, surrounding the ore, and is changed to a mass of lime silicates admixed with bunches of impure calcium carbonate. Where unaltered by surface agencies quartz, gray pyroxenes, and light-green amphibole appear to make up most of the rock. Some of it is extremely tough and resistant to fracturing. Where affected by surface agencies it is a crumbly granular sand of gray color. The ore-bearing part of the limestone can always be told by the alteration.

The scheelite is a gray-colored variety and most of it is so intimately admixed with the gangue in such small particles that they cannot be seen, and it is difficult to tell which is ore and which is not. Only by panning or assaying can the valuable portion of the ore be outlined. In a few places the scheelite may be seen with the naked eye in round grains the size of a pea, but if the rock is decomposed even these cannot be discerned, and it is only by experience of considerable panning tests that the scheelite-bearing material can be picked out. The part containing scheelite yields upon surface alteration a more sandy material than the non-scheelite-bearing material. There is an absence of veinlets, lenses, or drusy-filled cavities, of scheelite, and it is almost impos-

sible to obtain a hand specimen in which the mineral can readily be seen or examined without the use of a lens. This finely disseminated character of the scheelite will necessitate fine crushing for its extraction and consequent losses due to sliming, but the absence of other high specific gravity minerals should give a pure concentrate.

The ore appears to be in the form of small shoots or lenses within the limestone bed, which dip with the bedding at an angle of about 35°. Two such shoots are encountered in the shaft, but their size and extent could not be determined because of insufficient workings. The part of the upper shoot, as exposed, varies in width from 4 inches to 5 feet and is exposed along its dip for about 60 feet. It is said that the ore stoped from this shoot had a width of 4 feet. The average width of all the ore would probably be about 2 feet. The lower shoot has been exposed for a length of about 20 feet and in this distance varies from 1 to 6 feet. All of the ore within the shoots is the altered variety described above, but beyond the limits of the shoots it is a dirty gray crystalline limestone.

Sampling shows that the scheelite is irregularly distributed throughout the shoots and that small pockets and layers contain much more of the mineral than the adjacent layers. These, however, could not be separated in mining.

ORIGIN.

The scanty exposures of the deposit afford only a suggestion of the nature of its origin. It may be clearly seen, however, that it is not a typical contact-metamorphic deposit. The character of the alteration of the crystalline limestone and the nature of the minerals making up the deposit suggest that it has been formed as a replacement of the crystalline limestone by means of hydrothermal solutions. The pyroxenes, amphiboles, and other minerals noted in the ore suggest that the solutions were of high temperature. They probably were derived from the porphyritic granite intrusion near by. There is, however, as yet no evidence of direct connection between the two.

THE REPLACEMENT OF WOOD BY CALCITE.

C. W. GREENLAND.

A replacement phenomenon often observed in wood is that of silicification, and now so well known as scarcely to attract attention, but its replacement by other non-metallic compounds is less frequent, and hence the occurrence of a case of complete calcification seems worthy of record.

The material described here was collected by C. A. Warner from a point six miles north and two miles east of Russell, Kansas, and sent to the Geological Department at Cornell University.

Mr. Warner states that the fossilized wood here described formed the center of a three-foot concretion of lime carbonate, the latter lying in a bed of shale about 30 feet thick, with a bed of limestone about 10 feet above the concretion.

The shale occurs in the Fence-post horizon of the Benton.

The specimen studied (Plate VIII.) is approximately 6 inches long, and as stated above was broken from a three-foot concretion, a portion of which still in part surrounds it, and in substance looks like a dense, hard, gray limestone.

The sample at first glance resembles lignitized wood, but its weight quickly dispels this idea, moreover the application of hydrochloric acid to any part of the trunk causes violent effervescence. If a splinter of the wood is held in the flame, the brown color disappears completely. The surface of the trunk shows three prominent knots, and also a calcite-filled cavity which was formerly a resin vesicle.

There are also several transverse and radial fractures (Plate VIII.) up to an eighth inch in width, which are filled with coarsely crystalline calcite. A transverse section shows the annual rings quite clearly in the hand specimen, but they are less distinct under the microscope.

Microscopic Characters.—Thin sections of the trunk were cut both transversely and longitudinally (tangentially), and these when examined under the microscope showed the cell structure to be perfectly preserved (Plate VII.) permitting its identification as that of a gymnosperm. The entire mass of tissue however was filled with crystalline calcite, and no other mineral was observed. The original form of the wood cells is well preserved, as shown by their outline of organic matter. The medullary rays are very conspicuous (Plate VII.), and do not appear to have been replaced, as under crossed nicols they are isotropic; the annual rings are also recognizable.

While, however, the mass is completely replaced by calcite, the lime carbonate seems to have been introduced at two different periods, and so we can distinguish between cell calcite and vein calcite.

The cell calcite has completely permeated the woody tissue, not a cell being free from it, but the orientation is not uniform, for large areas of adjoining cells extinguish simultaneously (Plate VII., *C, D.*). In no case however was any individual cell found to be filled by calcite of two different orientations. Moreover, whenever a change in orientation occurs, it always takes place along the boundary of one or more cell walls. Indeed in several cases the change took place at the boundary between two annual rings.

Cleavage in the calcite was also visible, and was found to be uniform throughout an area of uniform extinction.

In places where the section happens to cut across a gnarled piece of wood, a grating structure is produced.

The vein calcite is evidently of later age, for it occupies fractures in the wood, sometimes enclosing fragments of vegetable tissue broken off the walls of the fracture. There is no agreement in optical orientation between the vein and the cell calcite, nor are the cleavage lines of the two continuous.

Manner of Calcification.—In considering the conditions under which the infiltration of the calcite took place, it seems reasonable to conclude that the wood probably exhibits a case of rather quick

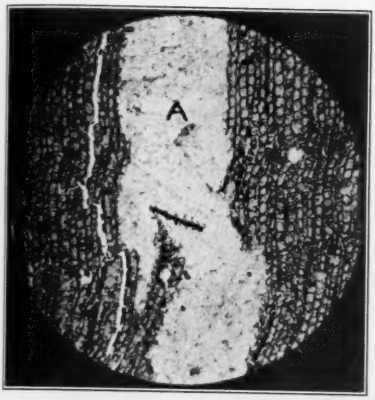
EXPLANATION TO PLATE VII.

A. Transverse section of calcified gymnosperm, × 50. Shows wood cells or tracheids; medullary rays; radial veinlet of coarse calcite (*A*), containing isolated fragment of medullary ray.

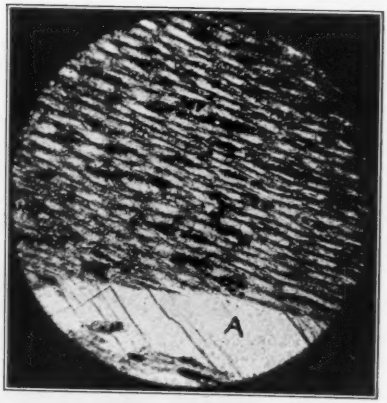
B. Longitudinal section (tangential) of Plate VIII., × 50. Shows medullary rays; wood cells or tracheids; calcite veinlet (*A*) showing cleavage.

C. Transverse section of Plate VIII. Crossed nicols, × 42. Shows areas of calcite having uniform orientation, change in orientation along annual ring; isotropic character of medullary rays.

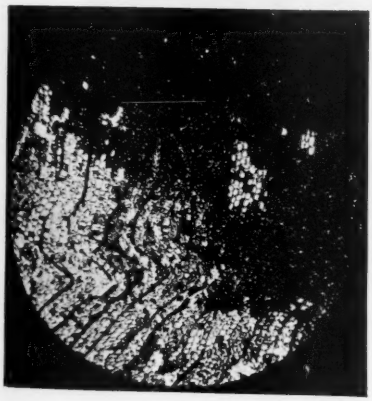
D. Transverse section of Plate VIII., crossed nicols, × 42. Shows change in orientation of cell calcite, and especially along annual ring.



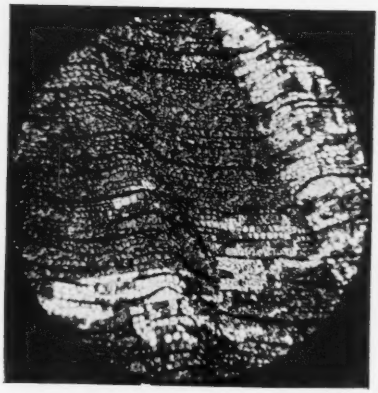
A.



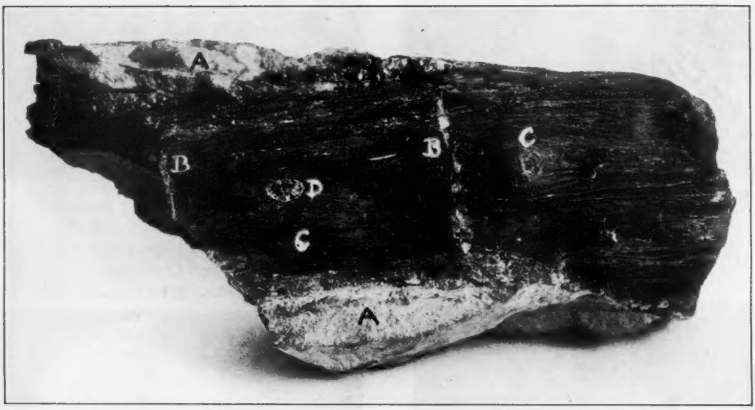
B.



C.



D.



Stem of gymnosperm, replaced by calcite. The specimen shows clearly the woody structure, a portion of the surrounding limestone concretion (A); a transverse veinlet of calcite (B); two knots (C); and calcite-filled pitch vesicle (D). ¾ natural size.

and complete burial while green in a limey clay or silt, and that the calcium carbonate entered the tissue before lignitization had a chance to begin. The very slight deformation apparent in the specimen would also indicate rapid entrance and crystallization of the calcite, while later pressure fractured the hardened trunk, and permitted the entrance of the vein calcite.

The writer knows of no similar instance of complete calcification, the nearest approach to it being a case described by E. T. Wherry.[1] There, however, the wood was first replaced in part by a little calcium carbonate in the form of rhombs, the remainder of the wood being subsequently completely silicified. In the growth of the calcite rhombs, replacement seems to have occurred only during the early stages of their growth, the later portion having pushed the vegetable tissue aside.

[1] *Proc. U. S. Nat. Mus.*, Vol. 53, p. 227, June, 1917.

AN OLD EROSION SURFACE IN IDAHO: IS IT EOCENE?

JOHN L. RICH.

The value of physiography as a means of determining recent earth history in a region where satisfactory sedimentary records are lacking needs no elaboration. Recently physiography is coming to be used as a means of dating ore deposits and in the elucidation of problems of secondary enrichment. If, however, topographic surfaces or peneplains are to be used in correlation, their correct dating becomes a matter of the greatest importance, and the evidence on which the dating is based cannot be too closely scrutinized.

Of late several papers[1] by Joseph B. Umpleby and W. W.

[1] Umpleby, Joseph B., "Geology and Ore Deposits of Republic Mining District, Washington," Wash. Geol. Surv., Bull. No. 1, 1910.

"An Old Erosion Surface in Idaho; Its Age and Value as a Datum Plane," *Jour. Geol.*, Vol. XX., No. 2, pp. 139–147, 1912.

"An Old Erosion Surface in Eastern Utah; Its Age and Value in Time Determination," Abstract, *Wash. Acad. Sci. Jour.*, Vol. 2, pp. 109–110, 1912. (This is practically a restatement of the material given more fully in the preceding reference.)

"Geology and Ore Deposits of Lemhi County, Idaho," U. S. Geol. Survey, Bull. 528, 1913.

"A Preliminary Account of the Ore Deposits of the Loon Creek District, Idaho," U. S. Geol. Survey, Bull. 530, pp. 67–68, 1913. ("The area is a part of that broad region known as the Salmon River Mountains and preserves upon its highest parts the Eocene erosion surface from which those mountains were carved. These remnants constitute the most significant geologic datum plane in the region.")

"Some Ore Deposits in Northwestern Custer County, Idaho," U. S. Geol. Survey, Bull. 539, 1913.

"The Old Erosion Surface in Idaho," *Jour. Geol.*, Vol. XXI., 1913, pp. 224–231.

"Ore Deposits in the Sawtooth Quadrangle, Blaine and Custer Counties, Idaho," U. S. Geol. Survey, Bull. 580, pp. 221–249, 1914.

Atwood, Wallace W., "The Physiographic Conditions at Butte, Montana,

Atwood have appeared in the journals and in Geological Survey reports in which an old erosion surface, or peneplain, recognized by various writers over large parts of western Montana, northern Idaho, and eastern Washington, is definitely referred to the Eocene and is offered as a valuable datum plane in a region where time relations are otherwise obscure. This datum is used in determining the date of the mineralization of certain ore deposits of Idaho, and the date of copper enrichment in the Butte district.

In view of the voluminous literature based on what is probably an erroneous determination of the age of the summit peneplain of eastern United States, it behooves physiographers to examine carefully the evidence offered in support of the definite dating of another similar widespread erosion surface. Inasmuch as the Eocene age of this peneplain has been questioned,[2] a reexamination of the evidence seems to be imperative.

SUMMARY STATEMENT OF THE PHYSIOGRAPHIC HISTORY OF THE NORTHERN ROCKY MOUNTAIN REGION, ACCORDING TO UMPLEBY AND ATWOOD.

Briefly stated, the physiographic history of the northern Rocky Mountain region as interpreted by Umpleby and Atwood has been as follows:

After intense deformation and mountain building at the close of the Cretaceous, a long period of quiet ensued, during which the region was reduced to slight relief. This old erosion surface is thought to have been completed during Eocene time, and is, therefore, called an Eocene erosion surface or peneplain. About the close of the Eocene, this peneplain was uplifted, deformed sufficiently to block out the main drainage lines, and was then

and Bingham Canyon, Utah, when the Copper Ores in These Districts were Enriched," ECONOMIC GEOL., Vol. XI., pp. 697–740, 1916.

Atwood, W. W., "Physiographic Conditions and Copper Enrichment," ECONOMIC GEOL., Vol. XII., pp. 545–547, 1917. (A reply to criticism by Blackwelder.)

[2] Blackwelder, E., *Jour. Geol.*, Vol. XX., pp. 410–414, 1912; ECON. GEOL., Vol. XII., pp. 541–545, 1917.

dissected by streams until broad valleys, 5 to 15 or more miles in width and 3,000 to 5,000 feet deep, were developed—presumably during the Oligocene epoch. Lakes whose shore lines may still be recognized were formed in these broad valleys and lacustrine and volcanic sediments accumulated to depths ranging up to several thousand feet, probably under conditions of semi-aridity at a time when the region had no direct drainage to the sea. Later, by stream capture, the region became opened to outside drainage and the basin deposits were dissected to their present condition. Umpleby maintains that Miocene and later eruptive rocks, also, were poured out into these valleys, nearly or quite filling them in places.

The argument for the Eocene age of the peneplain is that the Miocene "lake" sediments, and lavas also supposed to be Miocene, lie in intermont basins or valleys cut *after* the completion of the peneplain. That the basins could not have been formed previous to the peneplanation is thought to be proven by the presence of the shore lines of the Miocene lakes within the basins.

If the basins were formed *after* the peneplanation, and *if* Miocene shore lines still exist round the margins of the Miocene sediments within the basins, there seems to be no escape from the conclusion that the peneplain is Eocene. *But, does the published evidence support these postulates?* For the critical evidence the original reports must be examined. These are: Umpleby's report on the Republic District; the report on the Ore Deposits of Lemhi County, Idaho, and similar reports on adjacent or nearby regions; and Atwood's report on the Butte district.

Examination of Evidence of Eocene Age of Old Erosion Surface, Republic Mining District, Washington.[3]—The Republic Mining District is located in northeastern Washington, near the International Boundary. It is a rugged region, but the summits rise to a more or less uniform altitude.

After mentioning the general accordance of the mountain summits which suggests an old erosion surface, Umpleby dates

[3] Wash. Geol. Surv., Bull. No. 1, 1910.

this surface as Eocene, following Dawson[4] who has assumed an Eocene age for a similar surface in the interior Plateau of Canada.[5]

As proof of Eocene age:

> The early Miocene deposits of the Republic area lie in a great valley which was cut in the Eocene peneplain by a late Eocene or early Miocene river. . . . From this relation it is obvious that the peneplain surface is pre-Miocene, and from the fact that the early Miocene beds lie unconformable above earlier Tertiary lavas occur in a deep, steep-sided valley, it seems altogether probable that the peneplain is Eocene.

Umpleby begins with the assumption that the peneplain is Eocene, and follows this by the further assumption that the basins in which the Miocene sediments lie were cut by erosion in this upland surface since its formation. This second assumption is then used to prove the first.

The possibilities suggested by Blackwelder,[6] namely, that the Miocene sediments may have been faulted or bowed down among older rocks previous to the peneplanation, or may have been deposited in basins which were filled before the peneplanation but in some way came to lie below grade level at the time of peneplanation, are not eliminated or even seriously considered. Inasmuch as the field relations described by Umpleby seem to fit in more readily with Blackwelder's interpretation than with Umpleby's, the unproved assumption that the basins are younger than the peneplain can have no critical value.

On page 19 is the following statement:

> The Tertiary beds occupy a deep, broad valley cut in the older rocks after the elevation of the Eocene peneplain.

The evidence cited in support of this is the arrangement of the Tertiary sediments in long narrow belts and the fact that they rest against older rocks through a considerable vertical range

[4] Dawson, G. M., "On the Physiographic Geology of the Rocky Mountain Region in Canada, etc.," *Trans. Roy. Soc. Canada*, 1890, Sec. IV., pp. 1–74.

[5] In a recent report on the geology of a portion of the Interior Plateau ("The Beaverdell Map Area," Geol. Surv. Can., No. 65, Geol. Series, 1915). Reinecke assigns the peneplanation to the late Pliocene.

[6] *Jour. Geol.*, Vol. XX., p. 411, 1912.

as if deposited in steep-sided valleys. The possibility of faulting is mentioned, but it is stated that there is no direct evidence of it, and yet the structure section accompanying the geological map shows relations which might be explained much more readily by faulting than by deposition in a steep-sided valley.

The field relations described elsewhere in the report do not seem to bear out the interpretation of the troughs as post-peneplain erosion valleys. For example: The lava flows and dacite conglomerates at the base of the Tertiaries in the basin have dips of from 15 to 80 degrees in various directions.

> The formation is folded, but very much less intensely than the Paleozoic. . . .

Overlying the dacite conglomerates and unconformable with them, being very much less deformed, are Miocene "lake beds" in part thinly laminated, having a dip to the east of about 23° (p. 24).

In an attempt to harmonize the steep dips and the rather intense folding of the Tertiary sediments and lavas with the theory that they were deposited in a valley developed by erosion in a peneplain whose surface is still essentially undisturbed, Umpleby makes the following statement:

> The degree of folding which the dacite flow conglomerate possesses and which is vastly more than that of the Eocene peneplain might be thought to signify a pre-Eocene age. On the other hand, its known exposures, except along the ore zone where elevated by later intrusions, are all near the bottom of an Oligocene stream valley, and therefore must be younger than the Eocene.

Note the use of the assumption that the peneplain is Eocene, and the further assumption that the basins are later than the peneplain, and, therefore, must be Oligocene.

To account for the observed facts of distribution and position of the Tertiary beds on the basis of an Eocene age of the summit peneplain, the following series of events must be postulated: first, peneplanation in the Eocene; second, the erosion of a long trough-like valley 5 miles or more in width and several thousand feet deep; third, the deposition of dacite conglomerate and dacite

flows in this valley; fourth, the rather strong folding of these flows within the valley, yielding dips that must have ranged at least up to 57 degrees (80°–23°) and throwing the dacite conglomerates into well-marked anticlines and synclines as described; fifth, the erosion of these folded sediments within the valley; sixth, the deposition, unconformably upon them, of thinly laminated lake beds which must, from their nature, have been deposited nearly horizontally; seventh, the tilting of these lake beds to the east at an angle of about 23°, and finally, the dissection of the whole to the present condition.

Bearing in mind that, according to Umpleby's theory, all these events must have taken place in an erosion valley carved in an Eocene peneplain which is still recognizable and only slightly deformed, one may judge the validity of the evidence for the Eocene age of the peneplain in this district.

Lemhi County, Idaho.[7]—If the evidence from the Republic District fails to prove the Eocene age of the peneplain, what of that from Lemhi County, Idaho, which, with the Republic District, formed the basis of Umpleby's paper on "An Old Erosion Surface in Idaho: Its Age and Value as Datum Plane"?[8] Is the evidence from Lemhi County more convincing on the two critical points of the discussion—the relative ages of basins and peneplain, and the Miocene shore lines within the basins?

The principal geographic features of the county are indicated on the accompanying sketch map (Fig. 26). They are the mountains, the two intermontane troughs, and the valley of Salmon River.

The old erosion surface into which the present topography is carved is correlated with similar surfaces which have been mentioned by numerous workers in surrounding districts. The upland surface, of which considerable flat remnants are still preserved in places, has a general elevation of 8,500 feet, but here and there rises as much as 2,000 feet higher. As to the nature of the surface:

[7] U. S. Geol. Survey, Bull. 528, 1913.
[8] *Loc. cit.*

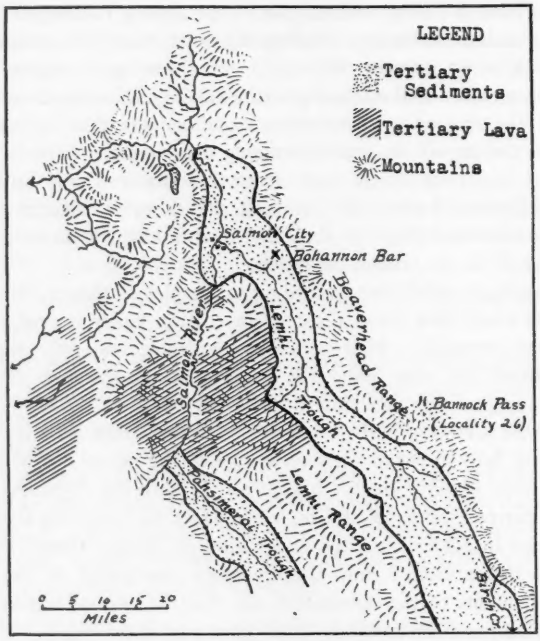


FIG. 26. Sketch map of Lemhi County, Idaho, after Umpleby, U. S. G. S. Bull. 528. Stippled areas are flat-bottomed intermontane troughs. The remainder of the area is mountainous, with the streams in narrow canyon-like valleys.

Were the flat-topped divides of the area used to determine a plane it would be found to record minor flexures, some faulting, and a broad anticline extending east and west through the Texas District. Such a plane, however, would probably prove much more nearly level than the structure of the Miocene lake beds would indicate.

As to the dating of the plane, we find the following:

In Canada and eastern Washington the surface has been recognized as Eocene and assigned to the same epoch as that in Lemhi County, which in turn is continuous with the west-central Idaho and Clearwater Mountain areas. The other localities mentioned above presumably pre-

sent the same surface, a deduction resulting from considerations outlined in the next section.

These considerations are:

Sediments assigned to the Miocene on fossil evidence occupy a broad erosion valley developed after the area had assumed its present elevation. Obviously, the old erosion surface is pre-Miocene (p. 26).

What is the proof that the "broad erosion valley" was developed after the peneplain? Umpleby reaches this conclusion by a process of elimination. Of the three possibilities, erosion, down-folding, and down-faulting, erosion is believed to be responsible. Down-folding is rejected because of lack of direct evidence and the fact that the basins lie athwart the structure axes of the region. The possibility of faulting is considered, but is disposed of in the following way:

If, however, the valleys are due to down-faulting they must have been blocked out prior to the deposition of the lake beds and after the elevation of the Eocene surface, for the lake which occupied the Lemhi Valley is shown by its shore line to have stood at its maximum 2,800 feet beneath the plateau surface (p. 36).

Thus, even if the valleys are due to faulting it does not invalidate the conclusion concerning the age of the Eocene erosion surface.

The present upper limit of the Miocene sediments is taken by Umpleby to mark approximately the elevation of the Miocene shore line in the basin, because:

The elevation to which the lake beds lap up on the older rocks is essentially the same on the narrow and on the wide divides between streams which cross the contact—an accordance which, as the rocks are of diverse resistance to denuding processes, should not prevail if the upper littoral deposits have been greatly eroded at these places.

Another reason for believing that this is approximately the old shore line is that:

In the vicinity of the divide between Lemhi River and Birch Creek, where even in the central portion of the valley the Miocene deposits are but slightly trenched, the marginal lake beds are essentially the same distance (2,800 feet) beneath the plateau level as at Salmon City, where present streams have cut into them to a depth of 1,800 feet.[9]

[9] In the description of certain of the mines of this region the Miocene beds are said to be in contact with the older rocks on the east side of the basin by a fault with a displacement of at least 300 feet.

Any determination of the level of the Miocene shore line on the basis of the present upper limit of the Miocene sediments seems to be completely vitiated by the structural relations of those sediments to the basins in which they lie. Thus, near the eastern edge of the Lemhi basin:

> On Bohannon Bar placer workings have exposed the eroded edges of the beds for about two miles, and throughout they show a uniform dip of 20° to 22° E. As a hasty traverse failed to reveal duplication by faulting, it is supposed that about 4,000 feet of beds are present in this exposure (p. 35).

That is, the beds are dipping at an angle of 20 or 22 degrees toward the base of the mountains which constitute the eastern border of the basin, and about 4,000 feet of them are there exposed. Yet it is in this vicinity that the present upper limits of the sediments at the base of the mountains is taken to represent the Miocene shore line.

The beveling of the sediments by the present surface furnishes the clearest possible proof that the present upper limit of the sediments is an erosion surface developed subsequent to their deformation.

Thus, the critical argument for the Eocene age of the peneplain and against the pre-peneplain age of the basins, namely, the supposed Miocene shore lines, is wholly unconvincing.

Not only is there no convincing proof that the basins are younger than the peneplain, but the structural relations of the beds within the basins militates strongly against such a supposition.

Of the structure of the Miocene beds Umpleby writes:

> The beds dip in various directions but generally away from north-south axes. About Salmon they incline 20° to 25° E. along both the east and west side of the basin, but toward its center they incline north or south.

Thus, not only are the beds in these basins greatly deformed in a direction parallel to the basins, but they are also deformed along axes crossing the basins approximately at right angles.

If we are to accept Umpleby's conclusion that the valleys are

later than the peneplain we must be prepared to explain how a peneplain could have survived, without significant modification, earth movements which so greatly deformed sedimentary beds in an erosion valley cut beneath its surface.

As further evidence of the Eocene age of the upland surface, Umpleby describes "Tertiary" lavas which extend in a belt nearly across the county. Of these he writes:

> These lavas occupy summits and valleys, Salmon Canyon being cut in them most of the way from Salmon up to the mouth of Pahsimeroi River, and the highest summits on either side being capped by them (p. 47).

Then comes the following statement:

> All the lavas occupy valleys developed after the elevation of the Eocene erosion surface, . . . (p. 48).

The geologic and topographic map accompanying the report seems to indicate clearly that these lavas form an integral part of the rock basement into which the present topography is incised. (See Fig. 26.) Moreover, the old erosion surface of supposed Eocene age is as well developed, with as large flat areas on the summits, across the supposedly Miocene lavas as it is elsewhere, and at the same levels. Very possibly those lavas were originally deposited in valleys, but that those valleys were carved by erosion in a peneplain surface which is as well developed upon them as elsewhere is not easy to conceive.

There are at least two lines of purely physiographic evidence which do not harmonize with the idea that the basins are younger than the peneplain.

The course of Salmon River with respect to the basins is significant. After emerging from a mountain canyon the river enters the northwestern end of the Pahsimeroi trough, crosses it, and again enters the mountains through which for a distance of 25 miles it flows in a wild, narrow canyon cut in resistant rocks. On leaving this canyon it enters the northern part of the Lemhi trough, here about 13 miles wide, follows it northward for about 20 miles to its extreme northern end, and enters a sharp, narrow canyon so rugged that for much of its length it is

practically impassable. In this canyon it flows about 250 miles to the border of the plateau region. Tributaries of the canyon have dissected the upland to early maturity or maturity, making everywhere an exceedingly rugged region with narrow, V-shaped valleys.

Thus it appears that there is an absolute lack of physiographic harmony between the course of Salmon River through these basins and its course elsewhere in the plateau, and this, as Umpleby is careful to state, is not explainable by differences in rock resistance.

If the broad basins were excavated in the old erosion surface by rivers, why did the rivers desert them, and how, in the span of a single cycle, did they gain their courses across the mountain barriers from one basin to another? The rocks in which the narrow, canyon-like portions of the river valley are cut are of the same kind as those in which the wide basins lie. Here is Umpleby's explanation:

> The valleys now partly filled by Miocene sediments were undoubtedly formed by rivers which flowed southward and joined Snake River. . . . These relations prevailed until erosion began to operate on the lake beds, when the head of the basin was tapped by Salmon River. Since then the drainage has been to the north and west. The explanation of the restricted valley occupied Salmon River from a few miles above Salmon nearly to the entrance of the Pahsimeroi is not altogether clear but probably is to be explained by headward erosion (p. 30).

This theory involves what is probably a physiographic impossibility. Granting that Salmon River, gnawing headward 250 miles through the resistant rocks of the plateau, was able to tap the northern end of Lemhi Valley, how is it possible that further headward gnawing of the river, instead of following the weak Miocene sediments of the valley, should have developed a canyon 3,500 feet deep and 25 miles long to the Pahsimeroi basin, and, after tapping that, instead of following it, should have turned again into the resistant rocks, only to repeat the process with respect to other basins farther up stream? Such a theory is difficult to reconcile with the principles governing stream capture.

If, however, the broad basins were formed and filled previous

to the peneplanation, and if Salmon River gained its present course on the surface of the peneplain, its relation to the basins is readily explained by superposition.

The great width of the Miocene lake basins is further evidence against their formation in the present cycle. From all descriptions it appears that, except where by chance they occupy portions of the broad basins, all the rivers of the plateau region have narrow, V-shaped valleys. Only exceptionally are there valley flats. The Miocene basins, on the other hand, from 5 to 15 miles in width, if produced by river erosion, imply an old-age stage in the cycle. If, as Umpleby contends, these valleys belong to the present cycle, and, as he states, they are not determined by belts of weaker rock, what were the other streams of the region doing while these broad valleys were developing? Are we to believe that in the same length of time they were able to cut only narrow, V-shaped canyons, or that the plateau should have remained undissected?

In reports on northwestern Custer County, Idaho, and on the Sawtooth quadrangle, the old erosion surface is described but no new evidence vital to our problem is produced. In both regions Tertiary lavas are described which are believed to occupy old erosion valleys cut in the peneplain, but in both, as in Lemhi County, the peneplain seems to be as well developed across the lava areas as elsewhere.

The Butte District, Montana.[10]—In the latest and most elaborate paper dealing with the physiography of the Idaho-Montana District, Atwood describes for the Butte District physiographic features essentially the same as those reported by Umpleby for the region farther west, and interprets them in practically the same way.

The physiographic conditions in the Butte District seem to differ from those of eastern Idaho only in that the intermontane troughs are relatively more numerous and extensive, and that remnants of the old erosion surface are more scattered. The intermontane troughs are floored with deformed Miocene sedi-

[10] ECON. GEOL., Vol. XI., pp. 697–740, 1916.

ments which show by their distribution that they are only remnants of former much more extensive deposits.

In dating the various physiographic features, Atwood describes Oligocene and Miocene basin fillings, then:

> As the troughs in which these sediments rest were used as drainage lines during the cycle of erosion just preceding the period of filling, and following the uplift and deformation of the summit peneplain, the development of those troughs and of the associated valleys must have been accomplished during late Eocene or early Oligocene time. From this evidence it is clear that the summit peneplain was developed after the close of Cretaceous time, and before the close of Eocene time (p. 712).

Note that the entire argument for Eocene age is based on the unproved assumption that the basins were developed *after* the uplift and deformation of the summit peneplain.

The argument for the dating of the peneplain is the same as that offered by Umpleby, and is open to the same objections. No proof is offered that the basins may not antedate the peneplain.

The field relations described by Atwood and the series of events which they imply not only fail to yield convincing proofs that the basins are younger than the peneplain but they are so difficult to harmonize with that interpretation that it cannot be accepted without the best of proof.

The following arguments, though they involve a matter of judgment, and, therefore, may not seem conclusive, indicate that the basins are older than the peneplain, rather than younger.

If the basins were cut in the peneplain since its completion, could the latter reasonably be expected to have survived the events recorded in the history of the basins?

If the basins were produced by faulting, and if Atwood's dating is correct, the existing remnants of the peneplain must have survived and maintained their moderately uniform level over the entire district in spite of the following events: (1) shattering by faulting so extensive that nearly one third of the entire area was dropped down some thousands of feet; (2) a long period of aggradation during which the basins were filled to depths of hundreds, and even thousands of feet by materials said to have been

derived largely from the adjacent mountains; (3) a period of deformation and mountain growth during which the Bozeman beds were deformed and given dips of 15 or more degrees in various places and uplifted in some places 1,000 to 2,000 feet by faulting; (4) a period of dissection, still in progress, during which large portions of the basin fillings were swept away.

If, on the other hand, as one might judge from Atwood's description, the intermontane basins were developed mainly by stream erosion following an outlining of the drainage courses by faulting, there is presented the physiographic anomaly of peneplain remnants surviving dissection to a stage of the cycle sufficiently advanced for the formation of broad, comparatively flat-bottomed basins, 5 to 15 or more miles wide. Such basins could have developed only in a cycle carried to the old-age stage, and in the course of such old-age development it is very doubtful if any considerable peneplain remnants could have survived. Not only that, but these basins are said to be independent of the control of weak rock masses. Here is a further anomaly, for selective erosion may be counted on to have reduced the weaker rocks long before the attainment of old age. Any broad basins formed in this way should show the closest sympathy with the resistance of the rocks. As a matter of fact there seem to be good-sized remnants of the peneplain preserved even on such weak rocks as the Cretaceous and Laramie shales and sandstones (Three Forks folio, Montana).

Even if it be granted that, in spite of these anomalies, the basins might have been developed without the complete destruction of the peneplain, there still remains the long period, occupied by the filling of the basins and by the subsequent removal of a large part of this filling, during which the peneplain must have been undergoing continuous dissection. According to Atwood's map (Fig. 52, p. 710) the Bozeman beds, at their maximum, occupied over one third[11] of the entire area and, in places, were over 3,000 feet thick. Though in part made up of volcanic

[11] 38 per cent. as determined by measurement on an area of 1,870 square miles of the central part of Fig. 52.

materials, much of the filling is said to have been washed in from the adjacent mountains.

Are we to believe that, after surviving the denudation by which the wide valleys were produced, the mountain areas could supply material for from several hundred to several thousand feet of filling in adjacent basins having a total area over one third as great as that of the mountains themselves and still preserve considerable remnants of the peneplain? All this may be *possible,* but it is not *probable,* and the burden of proof is on those who maintain that the basins are the younger.

In reply to a recent criticism by Blackwelder,[12] Atwood,[13] in an attempt to prove the post-peneplain age of the basins, cites field evidence which he considers to "be most helpful in this whole discussion, . . ." and to favor the Eocene age of the peneplain, but which seems most decisively against such an age.

On Fig. 52 is shown a system of stream valleys draining southward through what are now passes in the Beaverhead Mountains. The valley crossing Beaverhead range at locality 26 is shown as joining with Lemhi Valley which it receives as a major tributary. The rock bottom of the notch in Beaverhead range is said to lie "at least 500 feet below the old summit peneplain which appears at either side of the pass in the crest of the range."[14]

The top of the Miocene beds in Lemhi Valley, at the west foot of the pass referred to, is said and mapped by Umpleby to lie 2,800 feet below the summit peneplain nearby, and the deposits are estimated to be 2,000 feet thick. The rock bottom of the valley in which they lie would, therefore, be 4,800 feet below the summit peneplain. But in the pass, according to Atwood, the rock bottom lies "at least 500 feet," or using the larger figure, 1,300 feet below the peneplain. Thus using the peneplain as a

[12] Blackwelder, Eliot, ECON. GEOL., Vol. XII., pp. 541–545, 1917.

[13] ECON. GEOL., Vol. XII., pp. 546–547, 1917.

[14] In the original paper the somewhat discordant statement is made that the alluvial deposits in this pass are about 1,000 feet thick. If this is so, their base may be taken, at a maximum, to be 1,300 feet below the summit peneplain.

datum, the bottom of the main valley, as shown and described by Atwood, is hanging 4,300 feet (or, using the larger figures, 3,500 feet) *above* the bottom of its tributary. It is inconceivable that valleys as broad as these should not have been at grade when formed. Both valleys are said to have been developed after the formation of the peneplain. Faulting might account for the present discrepancy in elevation of the rock beds of the two valleys, but it cannot account for their discrepancy with respect to the peneplain unless the valleys were cut and the faulting took place before the development of the peneplain. Could there be clearer evidence that, if the basins were ever parts of a stream system, they antedate the formation of the peneplain and that the peneplain is post-Miocene instead of Eocene?

In reply to Blackwelder's criticism, Atwood,[15] in urging that the peneplain cannot be post-Miocene, cites as evidence Umpleby's determination of Miocene lake shores:

> Mr. Umpleby has reported that lake-shore features cut during Miocene times about the margin of Miocene deposits yet remain. This would not be so if the region examined by him had been peneplained after the close of the lake period.

The only evidence of Miocene lake shores which could be found in Umpleby's publications is that which has been previously discussed. The validity of such evidence needs no further comment.

SUMMARY AND CONCLUSIONS.

The critical point in the determination of the age of the old erosion surface is the question whether the basins in which the Miocene sediments lie were formed, filled, and deformed before or after the peneplain was cut.

A detailed examination of the evidence not only fails to reveal proofs that the peneplain is Eocene, but it brings out many features which point toward the conclusion that the basins in which the Miocene sediments lie were blocked out, filled, and in the

[15] *Loc. cit.*, p. 546.

main, these sediments were deformed before the peneplain was cut.

The date of the peneplain, therefore, is probably late Miocene or Pliocene. Further critical examination in the field is needed before the date can be determined more exactly.

If the peneplain is later than the basins, the dating of the ore deposits of Idaho must be revised, and the series of events in the physiographic history of the Butte District, including those connected with the enrichment of the copper ores, demands an interpretation quite different from that given by Atwood.

DISCUSSION

This department has been established by the editors in order to afford to those interested in questions relating to economic geology an opportunity for informal discussion. Contributions are cordially invited either in the form of discussion of more formal papers appearing in earlier numbers or bearing upon matters not previously treated. Letters should be directed to the Editor, Sheffield Scientific School of Yale University, New Haven, Conn. The full name of the author should be attached to all communications.

MAGMATIC ORE DEPOSITS OF SUDBURY, ONTARIO.

Sir:—The Sudbury occurrences are of such profound interest to students of ore deposits that the two articles by Drs. Bateman and Coleman in your August issue have doubtless been studied by many in the hope of finding some convincing solution of the vexed controversy in regard to their origin. Dr. Coleman alludes to my recent work on the Insizwa deposits of South Africa, which to some extent resemble those at Sudbury, and he points out that I regard them "as undoubtedly due to gravitational segregation." At the time when Dr. Coleman wrote his article my reply to the discussion that ensued on the reading of my paper before the Institution of Mining and Metallurgy had not been published. Several speakers alluded to the Sudbury controversy and one geologist in particular, of high repute, was quite as confident that my view of the Insizwa was wrong as I am that it is right. Now I fully realized at the time my Insizwa paper was written that without a convincing solution of the Sudbury problem, the interpretation I have given of the Insizwa deposits would be open to the same controversy. My position in regard to the Sudbury occurrences is that stated by Dr. Bateman, namely, one whose field experience of the Sudbury occurrences is limited, so I consequently did what he has done, that is to say, investigate the literature pertaining to the deposits and study the rocks and ores by means of thin sections and polished

specimens. It soon became only too obvious that to correlate the phenomena it was necessary to get back to bed-rock principles and treat the whole subject as a problem in physical chemistry, taking the bare facts of observation as recorded in the literature and setting aside for the moment the interpretations put upon them by the various geological observers. My method of attack has therefore been on somewhat different lines from those pursued by previous writers and the results can scarcely fail to interest your readers. They are given very briefly in Bulletin No. 156 of the Institution of Mining and Metallurgy, while a fuller explanation of the analytical methods and principles employed and the conclusions reached is now in the press. Meantime it may interest your readers to have this solution of the Sudbury problem put before them, more particularly as it paves the way for the solution of a whole host of problems connected with the derivation of ore deposits from igneous magmas in all parts of the globe. For the sake of brevity I cannot do better perhaps than quote the very condensed account given in the Bulletin previously mentioned. In principle the explanation of the Sudbury phenomena is a very simple one and depends on the fundamental differences between the anhydrous silicate slags of the human smelter and the hydrous slags or rock magmas employed in Nature's smelting operations.

When matte smelting is done under pressure in silicate melts containing small quantities of water, such as the natural rock magmas, the matte that separates contains quantities of H_2S that vary in amount according to certain principles. In this respect, as well as in certain others, the matte differs from ordinary dry-smelted artificial matte. On consolidation the sulphur of this dissolved H_2S is converted into metallic sulphide by reacting with iron oxide in the basic silicates in such a case as Sudbury. Thus some of the silicates are broken up by the sulphide consolidation, and there is an *increase in volume* and quantity of the sulphides at the expense of silicates taking place after the main silicate consolidation, giving rise inevitably to silicate replacement.

The hydrogen of the H_2S becomes oxidized to water by means of the oxygen liberated from the iron oxide on its conversion to pyrrhotite, thus giving rise to that local appearance of hydrothermal alteration, including hornblendization, that has led investigators of the Sudbury oc-

currences to suppose that the ores had been introduced by aqueous solutions.

Abstracting bases, FeO in the Sudbury instance, from basic silicates necessarily leaves acid residues, the so-called acid extracts.

I may add that there are certain other expansions of the sulphides, the approximate magnitudes of which can be calculated, but which differ from those involved in pyrrhotite formation in that they cannot be compensated by the chemical reactions with the silicates, consequently these expansions are "taken up" mechanically and help to produce that brecciation and expulsion of the matte into the surrounding country, which is such a marked feature of the Sudbury deposits. They are surprisingly large, the peculiar consolidation phenomena thus appear partly as the result of "sulphur blasting" arising from a very interesting play on the well-known polymerizing properties of sulphur, in conjunction with the successive development of the affinities of the different metals for additional sulphur to form disulphides as the temperature of the sulphurized matte diminishes.

It also appears that the concentration of the gassed matte at the lower margin is not entirely a result of gravitative accumulation but the two-phase—gas-liquid—constitution renders the matte amenable to other physical influences. Applying the principles of physical chemistry to magmas more acid than the Sudbury eruptive leads to the conclusion that flotation of matte, which is more highly gassed in such magmas, followed by its conversion or "self-bessemerization" and pressure squirting into the adjoining country, is one of the most important combinations of processes in the formation of deposits of the contact-metamorphic type, a type to which, as W. H. Weed tells us, an ever-increasing number of deposits is being referred.

One of the objections that has been raised against the smelting hypothesis for the Sudbury deposits is the supposed age relationship of the granites encasing the eruptive. Knight[1] discusses this matter as follows:

Dikes of coarse-grained granite penetrate the norite. Hundreds of them have actually been counted. . . . The age relationship is so clear

[1] Knight, C. W., *Eng. and Min. Jour.*, May 6, 1916, pp. 811 and 812.

that it would be unnecessary to emphasize the fact (sic) had it not been denied. They penetrate it hundreds of feet. . . . Clearly then the molten sulphides could not haye settled to the bottom of the norite magma . . . for the very good reason that the granite was not there when the norite was erupted.

Now at Insizwa we have a very similar structural arrangement. The eruptive is encased in acid sediments that are intensely metamorphosed for a distance of several hundreds of feet from the contact. The fine-grained recrystallized acid rocks in the neighborhood of the contact are scarcely recognizable as altered sediments were it not that the stratification again makes itself obvious on weathering, but running up from these fine-grained acid sediments into the ore-bearing eruptive there frequently occur a multitude of fine-grained acid dikes having sharp, well-defined walls in the eruptive but fading away imperceptibly into the encasing sediments. The whole general facies of the marginal structure is similar to that at Sudbury but on a smaller scale as regards size of crystals, size of dikes and size of eruptive. There is no doubt about the age of the Insizwa sediments. They are earlier. The question therefore arises as to what is the correct interpretation of these granitic dikes.

I attribute their formation at Insizwa to differential melting of the more fusible constituents in the encasing sediments and squeezing of the interstitial liquids so made into marginal cracks in the eruptive. It is generally admitted by geologists that the solidifying temperatures of acid rocks such as granite are considerably lower than those of basic melts.

It is probale therefore that at Sudbury earlier granites which may have been in contact with the eruptive at the time of injection became heated over a limited marginal zone to temperatures above the solidifying temperature of granite. But the solidifying temperature range of granite is very different from its melting range. All the evidence as to the constitution of rock magmas goes to show that this great difference between solidifying temperatures and melting temperatures is due to the excessive lowering of the freezing range by gases. The great geological agent for melting or lowering the freezing range is hydrogen

and its compounds, of which H_2O is the most important in silicate melts. If therefore we introduce into a mass of granite at tolerably high temperature and pressure hydrogen and its oxide we shall tend to melt a certain amount of it. Now the Sudbury magma was rich in hydrogen and its oxide as is evident from the rock analyses. It can be shown that in an iron-rich magma, such as the Sudbury, much of the hydrogen appearing as water in the rock analyses exists as free hydrogen in the liquid magma while it is well known that elemental hydrogen diffuses readily and fairly rapidly through solid silica, and probably even more readily through solid silicates. There is in fact no real chemical difficulty in understanding partial melting of encasing granite in a marginal zone in close proximity to the eruptive. I have carefully investigated the volume changes accompanying the freezing of a hydrous melt and it appears that at a certain stage in the consolidation of large masses of such melts the earlier-formed and solid shell is stressed by a bursting pressure produced by certain physico-chemical adjustments that take place in the partially consolidated interior of the mass. Thus at a fairly advanced stage in the consolidation of a large mass of igneous rock we have an outer solid shell shrinking on an interior mass that tends to expand to some extent at a point on the cooling curve. Now a bursting pressure exerts itself in such a way as to produce strains at a lower margin both downward and tangential. The mechanism of the formation of the Insizwa marginal cracks does not appear to have been quite of this order, but arose from volume changes resulting from the very rapid fall in temperature that takes place in a very narrow marginal zone highly preheated by the invading magma.

It seems not improbable that this kind of multiple small cracking was the forerunner of the larger cracks at Sudbury now filled with coarse-grained granite. The small cracks formed at the earlier stage being as it were the thin edge of the wedge for initiating the larger fracturing necessary to compensate the later-produced bursting strains. Thus at the lower margin of such a

mass as the Sudbury eruptive are produced conditions that tend to produce fairly large open fractures more or less at right angles to the periphery and at the same time tangential shearing.

These stresses and strains adjust themselves partly mechanically and partly physico-chemically but the important feature from the age point of view of the granite is that they would tend to drive partially melted granite into the fissures so produced in the outer shell of the norite-micropegmatite. It is probable also that this bursting pressure augments the "sulphur blasting" process and assists in "squirting" of the matte into the surrounding country.

In addition to this a portion of the floor of the nickel eruptive resting on a semi-fused mass, or say partly solidified granite, is a region that lends itself more readily to the compensation of bursting strains than parts of the eruptive resting on solid ground. Now the granite that sends these apophyses into the eruptive is described as gneissose in character and the gneissose character of granitic rocks is commonly considered to be due to movement of the melts when in an advanced stage in consolidation.

In order to understand the age relationships of the various Sudbury rocks it also seems advisable to pay a little more attention to the general course of the igneous cycle than appears to have been done by most writers.

The most striking feature of the early greenstones or sudburites, said to be the earliest members of the cycle, is their high iron content. The high percentage of FeO in the nickel eruptive is also a remarkable feature. In the following table I give the composition of a sudburite from Blezard, the average composition of 236 granites of all ages as given by Daly,[2] an average computed by mixing one part Sudburite and three of granite, and the average composition of the nickel eruptive as given by Coleman.[3]

[2] P. 19, "Igneous Rocks and their Origin."
[3] "The Nickel Industry," p. 106.

SiO_2	46.86	69.92	64.13	62.83
Al_2O_3	16.94	14.78	15.32	14.62
FeO	15.49	1.67	5.12	6.43
Fe_2O_3	4.18	1.62	2.26	1.25
CaO	9.65	2.15	4.02	4.18
MgO	2.94	.97	1.46	2.42
P_2O_5	.28	.24	.25	.16
TiO_2	1.54	.39	.67	.67
K_2O	.23	4.07	3.11	2.29
Na_2O	1.51	3.28	2.84	3.50
S	.09	—	—	—
H_2O	.47	.78	.70	1.12

Having regard to the variations in the chemical compositions of the sudburites as a whole and the rough and ready nature of the calculation, there is an astonishing similarity between the average composition of the nickel eruptive and a mixture of sudburite and granite. The suggestion is that the Sudbury magma was essentially a hybrid magma and it appears to have arisen largely by hydrogen stoping of sudburite through granite thus making a mixed melt. The hydrogen or its compounds, such as H_2S, possibly originated by emission from an inner metallic interior carrying a certain amount of nickel and copper with it, but this perhaps is going too deeply into cosmical speculation. The point is that granitic melting was part and parcel of the general igneous cycle, while even Sudbury geologists agree that some of the granitic intrusions preceded by but a short interval the eruption of the nickel eruptive. If the so-called later granites were in position but still fairly hot at the time of invasion by the norite, it would appear almost a foregone conclusion that they would send up granitic dikes into the norite at a late stage in its consolidation for the reasons that I have briefly given above.

I am therefore of opinion that those who maintain the later age of the granite masses as a whole on the ground of these apophyses sent up into the norite are as mistaken in their view as they are in their belief that the great ore deposits were formed by circulating solutions.

W. H. Goodchild.

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London, E. C.

DISCUSSION OF A PAPER HEADED "GENETIC CLASSIFICATION OF UNDERGROUND VOLATILE AGENTS."

Sir: In a paper in ECONOMIC GEOLOGY, Prof. R. A. Daly[1] says that I have erroneously stated that Posepny includes in the vadose circulation, water below the permanent water level. Professor Daly evidently had in mind Posepny's definition[2] rather than his usage of the term. The conception of the water level is a very helpful one if sufficient emphasis is given to its real character. It is not permanent, it is not a level; indeed it is a shifting, oscillating, undulating surface. It rises with the wet season and falls with the dry season, so that there is a zone, probably of considerable thickness in some places, which is below the water level in wet seasons and above the water level in dry seasons. This zone should be duly recognized in any treatment of underground waters, especially in the treatment of concentration of sulphide ores by underground waters. The oscillations of this zone, although "vadose," have deep-seated results. This may be illustrated in the diagram herewith (Fig. 27). After a dry season the water level will of course be low and the water near the water level will

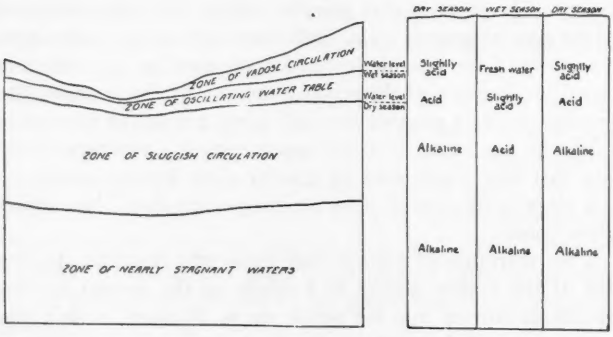

FIG. 27. Diagram showing chemical changes in the water of the zone of oscillating water table, and in the zone of sluggish circulation below it.

[1] Daly, R. A., "Genetic Classification of Underground Volatile Agents," ECON. GEOLOGY, Vol. 12, 1917.

[2] Posepny, F., "Genesis of Ore Deposits," p. 19.

be acid. At greater depth it will be alkaline. During the next wet season fresh water will raise the level of the water table, the resulting solutions will descend and a zone which has previously been alkaline will become acid by reason of the encroachment of the overlying acid waters. After longer contact with ores and rocks, however, the water again will become alkaline. It is in this zone of alternating alkaline and acid conditions where many of the changes take place in the enrichment of copper and silver ores. It is practically impossible to make synthetically under natural conditions the compounds of the silver-antimony-arsenic sulphides without recognition of these changes. By varying acidity and alkalinity it is easy, however, to form such compounds even in cold solutions in short periods. It seems to me that the oscillations of the water table have not yet been given the attention they deserve in classifications which have been proposed for underground waters.

W. H. EMMONS.

THE GEOLOGIST IN WAR TIME—GEOLOGY ON THE WESTERN FRONT.

Sir: The practical applications of the general principles of geology in the Great War have been described by Professor J. S. Ames, of Johns Hopkins University, who was sent abroad last spring by the National Research Council as chairman of a committee of six to investigate the application of science to war, as illustrated on the western front. Three paragraphs from his article "Science at the Front," *Atlantic Monthly*, January, 1918, will be of interest to geologists.

> Take geology. I had heard that geologists were attached to the Staff; but I had pictured them as mining engineers rather than as professors of the pure science. Imagine, then, my surprise when I found in one of the rooms at headquarters a world-famous geologist studying and marking areas on a geological map of Flanders. All this country through which the battle-line passes has been studied with care by geologists for many decades, and Belgium and France have both published sets of maps showing all the geological details. On the professor's table was a map of the district directly east of Ypres; he was coloring certain areas red and others various shades of blue. He was also marking certain points and drawing a few straight lines.

Naturally I asked what it all meant. One color meant "Here it is safe to make dug-outs"; another "Here you will strike rock"; another "Look out for quicksands"; and so forth. The points meant "Dig for water." The straight lines meant "Here you may make tunnels or burrow mines."

I saw on the walls of the room vertical sections of the country, and inquiry brought the answer that they were for the study of underground water-systems; for the rise and fall of such might interfere with tunnels and mines, and so knowledge of them is necessary. Never have I spent a more interesting hour. It was said that one reason for the great success of the British operations at the Messines Ridge, when fifty or more mines were exploded, was the skill of the geologist who planned their location; for in some cases they were so surrounded by quicksands that the Germans could not countermine. I cannot vouch for the truthfulness of this, but, personally, knowing the men concerned, I believe it.

In this connection the value of block diagrams, which show the relation of topography to geological structure, as an aid to the officer in visualizing the topography of a region in the theater of war should be pointed out. D. W. Johnson, in "Topography and Strategy in the War," has two such diagrams, both excellent, but one of which is in many respects ideal. It is a stereograph of a portion of the Paris Basin and contiguous regions to the north and shows the relation of topography to structure. A study of this diagram furnishes the student with a key to a large region and enables him to visualize the topography; escarpments, gentle slopes, position of forests, location of swamps and marshes, and other features of great military importance. Such a diagram must be generalized but its very simplicity gives the officer the structural basis of the topography, and thus enables him to interpret the minor details of topography not shown on the diagram. Well-made topographic maps and carefully constructed geological maps show everything, and more, than such a generalized diagram, but an officer seldom has the technical knowledge or the time to gather the necessary information in this way. Consequently, block diagrams which show essential topographic and structural features should be provided and it will be surprising if the great value of such diagrams for the training of officers is not appreciated by our military in the future.

HERDMAN F. CLELAND.

Sir:—The interesting paper by A. W. Lauer on "The Petrology of Reservoir Rocks and its Influence on the Accumulation of Petroleum"[1] discusses the openings in sedimentary rocks in their relation to oil segregation in a new light. Emphasis is laid upon what the author calls "induced" openings brought about by diastrophic forces, solution, etc., and the part played by "original" openings is reduced to a place of decidedly minor importance.

There are several points on which, it seems to me, the author's conclusions are open to discussion. For example, he gives as the size of capillary pores in rocks, less than .508 mm. diameter for tubular pores and less than .254 mm. cross-section for fissures. These figures are correct for water but the fact that the maximum size of opening in which capillary will affect the average oil is considerably smaller—.2 mm. diameter for tubes and .1 mm. cross-section for fissures—is not stated. This is an important point for the movement of oil brought about by the greater capillary force of water does not necessitate a movement of oil from pores of capillary size to pores of super-capillary size, but the movement is considered to be from pores of small dimensions to pores of larger dimensions regardless of the fact as to whether the latter are super-capillary in size or not. The interchange of water for oil in fine-grained materials, the oil moving to the coarser-grained materials even though the pores of the latter are still less than .508 mm. diameter, will take place.

Again, Lauer does not believe, apparently, that original openings in detrital rocks (with the exception of some coarse conglomerates) are ever of super-capillary size, *i. e.*, greater than .508 mm. diameter for tubes and greater than .254 mm. for fissures. But these are, as stated above, the maximum sizes for water. For average oil the sizes are in each case over 50 per cent. smaller. It is not only probable but is doubtless a fact that many porous sandstones have many openings or voids which are at least .2 mm. in diameter, the maximum capillary size for oil. If such a rock is in juxtaposition with a fine-grained rock saturated with oil and in the presence of water, there is bound to be

[1] Economic Geology, Vol. XII., pp. 435–472, 1917.

an interchange even though the pores of the coarser rock may be of capillary size for water.

Another point which suggests itself is the unreliability of basing conclusions, in so far as typical "sands" are concerned, as Lauer has done, on studies made on specimens of these "sands" which have been "blown" from "wells." In the first place consolidation in these "sands" is a very variable factor and these wholly solid pieces may not be and probably are not typical of the entire bed from which they come. Again we cannot hope to duplicate in the laboratory conditions of pressure and flow even approximating the conditions which may exist in a deeply buried stratum. It may appear impossible, looking at a specimen "blown" out of a well, that a bed of the same material saturated with oil could yield a flow and yet under the conditions existing at the well bottom, whether the specimen is typical of the bed or not, we are confronted with the fact that in many cases the flow does take place.

If "induced" openings in sandstones—we all must admit their importance in limestones—are so all-important, then why, the question may be asked, are not oil pools always located in areas where we would expect such openings to exist? "These would be prominently developed at the crests of folded areas . . ." and yet in the Pennsylvania fields the great majority of oil "pools" are not at the crests of folds but on their limbs. Perhaps the author would blanket this entire region with "induced" openings. Surface observation certainly does not contribute corroborative evidence. Except in highly folded regions, specimens of rocks taken from outcrops on the crests of anticlines certainly do not exhibit "induced" openings.

Finally, Lauer believes that a recognition of the importance of "induced" openings directly "reëstablishes" the "anticlinal" theory. In the first place the relation is not evident, especially in the Appalachian oil fields, and secondly the need of reëstablishing this theory is not apparent. The "anticlinal" theory is so firmly intrenched in the minds of oil men in general and is so little correctly understood that any paper tending to show that

the movement of oil is far more complicated than the simple process of gravitational separation is welcome. Washburne's valuable paper[2] on "The Capillary Concentration of Oil and Gas," while probably by no means the final word in oil movement, at least introduced a factor that will work and which must be considered and at any rate opened a new field of thought and investigation which is proving prolific in results.

WILLIAM F. JONES.

[2] *Trans. Am. Inst. Min. Eng.*, Vol. L., pp. 829–858, 1914.

REVIEWS.

The Gold Deposits of the Rand. By C. Boring Horwood. 8vo, pp. xviii, 400, figs. 64. London, Charles Griffin and Company, 1917.

All who follow the literature of mining geology are already familiar with the series of valuable papers which Mr. Horwood has contributed on not a few important phases of the great gold-bearing banket. The results set forth have now been collected, amplified, systematized and made into a volume complete in itself. The author has brought to his task excellent preliminary training in those branches of geology which are closely related to mining; he has read widely and with care; and he has gathered his observations and evidence at first hand during twelve or fourteen years' experience in the mines. Under these circumstances his work could not fail to be of exceptional value, and careful reading of its pages abundantly justified this impression.

The book opens with a foreword from Mr. T. A. Rickard. The foreword is followed by a preface and an introduction from the author himself. The introduction summarized the three views which have prevailed regarding the entrance of the gold and pyrite into the conglomerate. These are, first, the sea-beach placer; second, the sea-beach placer followed by re-solution and re-deposition; third, the introduction of the gold and pyrite from below, according to the most trusted views on the formation of lodes. The author is a firm believer in the third method and his great object is to establish this thesis.

In order that American readers may most intelligently follow a summary of the work, the geological section of the Rand may best be first given as the simplest way of setting forth the problem. The data have been collected and tabulated from the general and more scattered statements in the text. Since all the strata mentioned in the table are devoid of fossils, we may only state that they are certainly older than the Devonian, and that the belief is now general that the Witwatersrand system and those beneath it are Precambrian.

Systems.	Series.
Transvaal or Potchefatiom.	Pretoria, Dolomite, Black Reef.

Unconformity.

Ventersdorp. — Volcanic rocks and diabasic dykes, the latter frequent in the mines.

Unconformity.

Witwatersrand .....

- Upper 640 square miles, 10,000 feet clastics....
  - Elsberg, 6000 feet clastic with several conglomerates containing a little gold.
  - Kimberley, clastics with several conglomerates, mined on the Far West Rand.
  - Bird, clastics, no gold-bearing beds.
  - Livingstone, clastics, no gold-bearing beds.
  - Main Reef, 125–400 feet of quartzites and conglomerates resting on 2000–3700 feet of shales.
- Lower 970 square miles, 10,000 to 12,000 feet of quartzites and slates .....
  - The Lower Series embraces three conglomerates, viz.: *Government,* gold-bearing and promising; interval 1900 feet; *Coronation,* not profitable, interval 2750 feet; *Promise,* not profitable.

Unconformity.

Swaziland (Archean). Basement or Old Grey Granite (Archean).

Almost all the gold is obtained from two conglomerate beds, but there are others of local importance. The details of the Main Reef Series may be conveniently expanded as follows:

Main Reef Series above a great section of shales .....

- South Reef, sub-series .....
  - Hanging Wall Reef 9″–18″.
  - Quartzite 8″–12″.
  - Middle Reef 3″–5″.
  - Quartzite 8″–12″.
  - South Reef Leader 0.5″–6″.

Quartzites 40–200 feet.

- Main Reef, series proper .....
  - Main Reef Leader 3″–4″–24″.
  - Shale 3″–8″ persistent.
  - Quartzite up to 6 feet.
  - Main Reef 4″–5″–3 ft.–12 ft.

The chief gold-bearing bed is the Main Reef Leader and next to it in importance, but less uniform in values, is the South Reef Leader. The Main Reef itself is low-grade, usually 2–4 dwt. per ton, and is commonly 4–12 feet thick. The Middle Reef contains a few dwts. and is stoped with the South Reef Leader; but the Hanging Wall Reef is for the most part left behind. (These details are taken from pp. 194–196.) Thus the mines have two lodes, the Main Reef Leader and the South Reef Leader, separated in the run of practice by from 40′–120′ of quartzite.

Structurally the Witwatersrand System forms a great syncline with a major axis running northeast and over fifty miles long. The conglomerates are mined along the northern rim and with a southern dip. At the east and west ends of the syncline the beds bend around to the south and the productive conglomerates on the east have been treated by borings where covered by the overlying Black Reef and Dolomite series. Throughout the mining districts great dykes of diabase with parallel strike to the productive series have come up along strike faults. They are of various widths up to 400′. Cross dykes in the general direction of the dip are also known. Faults are likewise important features of the structure. The dykes indicate a great, deep-seated mass of igneous rock, originally their source.

The author in Chapter II. takes up the "Pyrite Replacements," because it is obvious if replacement can be shown, the introduction of the pyrite and its contained gold has taken place from an outside source. For many years past the existence in the bankets of rounded masses of pyrite known as "pyrite pebbles" has been a matter of observation and record. The advocates of the placer hypothesis considered them water-worn survivors of ancient sea-beach conditions. Our author, however, by means of miscroscopic study of thin sections clearly demonstrates that they are due to the replacement of original pebbles of quartzite and vein-quartz: of the matrix of the conglomerates and sometimes of both pebble and matrix in the same pyrite spheroid. The replacement of an original pebble may be but partial, leaving a core of the original; or may be indicated by unreplaced fragments which survive in the pyrite. In fact, after weighing the evidence, no candid reader can fail to conclude that the replacement advocated by the author is soundly established. The same subject is continued in Chapter III., with a theoretical and extended discussion of the physics and chemistry of the process; involving as it does at times concretionary and radiating structures. Many interesting points are brought out. The author is alive to the possibility of high temperatures which may have prevailed at the time of precipitation. On pp. 65–67 the shape and growth of the pyrite pebbles are discussed. They are predominantly spheroidal and ellipsoidal and in this

feature preserve the shapes of the replaced pebbles. The elongation is along the dip, leading to the inference that the solutions circulated most freely up the dip. The effect of the dissolving part of the process on angularities would be to smooth them down, because solution is believed to act more vigorously the greater the pressure. Obstacles in the way of free flow would tend to be dissolved and removed. Presumably, also, those portions of the pebbles which lay across the stratification, which is the line of flow, would stand the best chance of solution and replacement, while the flat sides in the planes of stratification would let the solutions pass by with greater ease. In a footnote to p. 67 the author takes the opposite view to the above statement, and seems to the reviewer to have fallen into a contradiction to the general principles.

In the discussion of the association of gold and pyrite, the author considers the possibility of eutectic solutions of the two, and even of eutectic gases. If in excess, the pyrite would be precipitated alone until the eutectic point was reached, when both would crystallize together, a suggestion over which all students of the gold-quartz veins may well ponder.

In Chapter IV. the author discusses the carbon in the banket, a subject on which he has written at length in earlier papers. The carbon appears in small, rounded and irregular masses, resembling dull graphite and is often associated with rich ore. Its dissemination is so fine and its mixture so intimate that satisfactory samples for analysis are not easy to obtain. From 15 lbs. of ore (p. 96) by fine crushing so as to pass a 90-mesh sieve, 19 grams of 0.54 per cent. carbon were obtained. From 45 lbs., by crushing, sliming in water, skimming and purifying with dilute nitric acid, 1.5 grams containing 16.5 per cent. carbon were obtained. Sir H. A. Miers tested another sample and could find little except carbon, associated with a sulphur (and arsenic) compound, probably of iron. The carbon mineral was not believed to be a hydrocarbon. From a coarser sample which could be separated by hand, Mr. Horwood obtained 3 grams of hardness between 2 and 3, sp. gr. 1.70; 49 per cent. carbon. Destructive distillation gave traces of oil, thereby proving the presence of hydrogen, not combined in water. The carbon mineral was scaly, flaky, and had experienced shearing. A different specimen gave similar results. From the central portion of two diabase dykes (p. 91), respectively 1,050 and 2,090 feet below the surface, fresh, undecomposed samples yielded to quantitative analysis, in addition to the usual components, carbon 0.22 per cent. and 0.04 per cent. Again from a diamond drill core of a dyke 230 feet thick, and at 114 feet from the edge, a sample of somewhat decomposed rock was taken and analyzed. Besides the usual components, 0.03 per cent. carbon was found. Another sample gave 0.2 per cent. These determinations, unusual as they are, are of

great scientific interest.[1] Mr. Horwood concludes that the carbon is derived from the deep-seated, diabasic magma, and that it has entered the bankets as has the gold, pyrite, quartz, and some minor associates, as an igneous emission. He reviews practically all the recent work on graphite in support of his view.

Chapter V. is another of extraordinary scientific interest in that in it our author discusses the rare metals of the platinum group which are found in minute quantities in the bankets. At the East Rand Proprietary Mines operating on the Main Reef Series and Main Reef Leader a half ounce and less of the rare metals to the ton of black sand concentrates were determined by the assays of A. F. Crosse. Up to 9 dwt. of the same per ton of black sands were found in the Klerksdorp Mine, and the presence of the platinum group has been noted in several others. With much expenditure of labor Mr. Horwood secured samples of these metals and had them spectroscopically examined at the Solar Physics Observatory at South Kensington. Osmium, iridium and ruthenium were the chief metals revealed. In one case rhodium, palladium, and platinum were possibly indicated in faint traces; in a second sample the last-named three failed altogether. Other elements whose lines could be recognized were Fe, Ca, Al, Cr, Ti, Si, Mg, Sr, Na, Mn and Va. A quantitative analysis of a sample was made by Professor W. R. Dunstan, of South Kensington, and yielded after the removal of quicksilver caught from the battery plates—

Gold	1.24 per cent.
Platinum	0.61 per cent.
Fe_2O_3	1.29 per cent.
NiO	0.66 per cent.
Osmiridium (Ir.Os and allied metals)	95.52 per cent.
	99.32 per cent.

The osmiridium yielded 45 per cent. iridium. The concentrates thus were essentially osmiridium. Now, the interesting question arises whether the rare metals are of placer origin or whether they have been introduced by the same agents which have brought in the gold. This question is discussed at length in Chapter VI., and the conclusion is reached by our author that the rare metals have been brought in by the same agents which have introduced the gold, *i. e.*, magmatic emissions from the deep-seated parent batholith of the diabase dykes.

The placer method of origin is not without supporters, and as we are dealing with gravels and small amounts of extremely rare elements, this

[1] They recall the determinations of small percentages of carbon by W. F. Hillebrand in Adirondack gabbros and titaniferous iron ores, as recorded in the paper in the 19th Ann. Rep. U. S. Geol. Surv., Part III., pp. 377–422, 1898.

view merits very serious consideration. If the osmiridium could be obtained without a previous passage through the stamp batteries and tube-mills, and could be examined with the microscope, the shape might give some tangible evidence. Were the flakes rounded and water-worn the placer view would have claims. Osmiridium, however, is so hard that it is crystalline and angular at times even in undoubted placers. The positive demonstration of introduction by vein-forming agents is less easy than for the gold, but Mr. Horwood reaches the conclusion that both have entered by the same processes. All evidence of the migration of members of the platinum group in solution is of exceptional scientific interest, but that they could have been vaporized or that the exalted temperatures which would be required could have existed in the formation of the lode, and far from the parent batholith as seriously considered on p. 136, seems to the reviewer to be scarcely conceivable.

In Chapter VII. our author departs from the purely scientific discussions of the earlier pages and takes up the first of a no less interesting series of economic topics, namely the life of the Rand Gold Field. An estimate can be made much more accurately than with the ordinary gold mine, since in their exploitation the bankets are more like coal mines than metalliferous deposits. The productive beds of conglomerate both at present and prospective have to be selected. The average profitable yield per ton both as shown by past experience and as likely with improved methods to be reached in the future, and both as shown near the surface and as to be expected with the usual lessening of values with depth, must be determined and decided with great care. The proportions of rich and poor ground for each of the bankets considered are important factors. We may quote Mr. Horwood verbatim from p. 186: "Taking a length of eighty miles, an average width of 5 feet, a vertical depth of 5,500 feet, an average of, say, 30 degrees; allowing only 50 per cent. for payability, taking the usual figure of 12 cubic feet of ore to the ton, and subtracting the total tonnage milled to the end of 1914, as given in the statement on p. 182,[2] and assuming an average recovery of 25s. per ton for the remainder, we obtain a total of 712,000,000 tons, and a total future recovery of £890,000,000 down to a vertical depth of 5,500 feet. This, with the amount of £417,000,000 won up to the end of the year 1914, gives a total recovery value of over £1,300,000,000.

"Taking the present yearly rate of crushing as, say, 26,000,000 tons, this alone would give a life of over 27 years; but as the rate of crushing after attaining a maximum would gradually decline, the life on the foregoing basis would be considerably prolonged."

In Chapter VIII. the distribution of the gold is discussed. Of necessity many interesting points must be passed over in this review, but the

[2] 255,894,526 tons.

chief resources, despite the several bankets, reduce to the Main Reef Leader and the South Reef Leader, with an interval of 40–120 feet between them. Both are broken in the same mines, but when one is rich the other opposite it is apt to be, or customarily is, poor. Some interesting curves of variation and averages are given, and also projections of rich and poor areas on the plane of the reefs. Some rich areas are described as "patches," others correspond with shoots and, as usual with shoots, have a diagonal pitch in the plane of the banket. The effect of neighboring dykes of diabase upon the yield is discussed and is regarded as favorable. The longitudinal dykes are more beneficial than the cross-dykes. The influence of faults upon the yield of gold is also discussed and some assay plans of areas next them show that they too exercise in instances an enriching effect.

In Chapter IX. the local indications of good ore are reviewed. Large pebbles in the conglomerate beds are apparently characteristic of the ones specially enriched with gold. They seem to have increased permeability for the gold-bearing solutions. Much pyrite is often, though not invariably, a good sign. The pyrite pebbles earlier cited are always a favorable indication. Carbon is also. The beds contain black-edged, pink and milk-white pebbles. Since all three were found by Mr. Horwood to be associated with good ore, he gave them careful microscopic and chemical study. All three peculiarities were believed to be due to changes in the pebbles subsequent to deposition. The cause of the black edges and sometimes the blackening of the entire pebble was found to be some extremely minute, diffused, black substance which could not be resolved under the microscope even by a high-power oil immersion lens. A long series of experiments on thin slices of the black-edged pebbles were conducted, embracing parallel quantitative analyses of black and ordinary pebbles; treatment with strong oxidizers to remove possible carbon (ineffectual); treatment at high temperatures up to 1000° C. for many hours (ineffectual); with strong acid and heat (gradual bleaching); and finally with spectroscopic examination alongside of parallel tests of ordinary pebbles. Mr. Horwood's conclusions were: (1) the coloring matter is due to the same agencies to which the gold is due; (2) it is caused by manganese and chromium, or their compounds, and also possibly by iron; (3) the coloring agents have been introduced at high temperatures, probably under pneumatolytic conditions (p. 244).

In Chapter X. the whole subject of the genesis of the gold and the period of mineralization is summarized and stated at greater length than in the opening pages. The evidence having been all stated, the author sums up the case. The parallelism of the bankets with ordinary veins and lodes is regarded as proved. It remains to outline the process according to our best modern views on vein formation. Emissions from

a deep-seated, igneous magma meet the conditions. The Ventersdorp diabase batholith, the parent of the many dykes, is believed to be the specific source, and the time following the breaking up to the surface of the dykes is the actual period of mineralization. It is possible that some lingering traces of the old deep-seated waters remain. At the Witwatersrand Deep Mine there is a fault along a dyke known as the water fault, which has been tapped to depths well below 2,000 feet. The water cannot well be vadose in origin, because lower levels cut off the supply of upper ones, and yet when first opened yield extremely high pressure and great volumes.[3]

Chapter XI. follows with a hundred pages of reprinted discussions, chiefly from the Mining and Scientific Press in connection with earlier separate contributions of the author. The views of many students of ore deposits on one phase and another of the Rand are thus stated. Chapter XII. concludes the book, with some final discussion of the general subject of the deposition of the gold.

The reviewer hopes that some idea of the extremely valuable and suggestive work may be gained from the above summary. Not a few minor points have been passed over with regret. They will, however, catch the attention of a careful reader. To all interested in the subject of ore deposits the book may be warmly commended as having been prepared after years of original, first-hand observation and patient study, and as containing clearly stated and carefully elaborated conclusions.

J. F. Kemp.

[3] These relations prove that the water did not come down the fault from the surface; but if fed in from the surface to the fault by some other conduit at a depth not yet reached, so that they rose in the fault, the above relations might obtain.

SCIENTIFIC NOTES AND NEWS[1]

SCIENTIFIC NOTES AND NEWS

THE GEOLOGICAL SOCIETY OF AMERICA elected the following officers for 1918: *President,* Whitman Cross; *First Vice-President,* Bailey Willis; *Second Vice-President,* Frank Leverett; *Third Vice-President,* F. H. Knowlton; *Secretary,* Edmund Otis Hovey; *Treasurer,* E. B. Mathews; *Editor,* J. Stanley-Brown; *Librarian,* F. R. Van Horn; *Councilors,* Joseph Barrell and R. A. Daly.

F. L. RANSOME, of the U. S. Geological Survey, has returned to San Francisco from an inspection of the quicksilver mines of California.

F. H. HAYES is Captain and Adjutant, First Arizona Infantry.

A. C. LAWSON has been elected chairman of the San Francisco section of the A. I. M. E. with W. H. Shockley as secretary and treasurer.

THE GEOLOGICAL SOCIETY OF LONDON, England, announces the following awards of medals and funds: Wollaston Medal, Dr. Charles D. Walcott, Smithsonian Institute, Washington; Murchison Medal, J. B. Tyrell of Toronto.

THEODORE CHAPIN, of the U. S. Geological Survey, is at Silver Lake, Colo.

C. K. LEITH, of the University of Wisconsin, and J. E. Spurr, of Philadelphia, are in Washington, investigating the ocean movement of mineral commodities for the U. S. Shipping Board.

[1] Geologists, mining engineers and others interested in applied geology are invited to keep the editor informed of new investigations of mining districts or scientific studies undertaken by them, together with such other scientific and personal items as may come to their notice.

R. A. DALY has gone to France in connection with Y. M. C. A. work.

GEORGE J. YOUNG, recently professor of mining at the University of Minnesota, has joined the editorial staff of the *Engineering and Mining Journal.*

THE FEBRUARY meeting of the American Institute of Mining Engineers was largely attended, notwithstanding the large percentage of members in service. The technical sessions were of unusual interest as the articles read had been planned to meet problems arising from war conditions. Papers were presented on "The Chilean Nitrate Industry," by A. H. Rogers and H. R. Van Wagenen; on the "Genesis of the Sudbury Nickel-Copper Ores as Indicated by Recent Exploration," by H. M. Roberts and R. D. Longyear; "Relation of Sphalerite to Other Sulphides in Ores," by L. P. Teas; "Pyrite and Pyrrhotite Resources of Ducktown, Tenn.," by J. H. Taylor; "Ore Deposits of the Yellow Pine Mining District," by F. A. Hale, Jr.; "Phosphate in Egypt," by E. Cortese; "The Wisconsin Zinc District," by H. C. George. The geology papers were discussed by G. F. Kunz, W. G. Miller, W. Lindgren, L. C. Graton, C. P. Berkey, A. M. Bateman, J. T. Singewald, Jr., W. O. Hotchkiss, W. N. Smith, and Sidney Ball; the petroleum and gas papers by A. F. Lucas, C. W. Washburne, W. E. Pratt, E. W. Shaw, Dorsey Hager, and Eugene Coste.

POPE YEATMAN has been placed in charge of the non-ferrous metals department of the War Industries Board.

DOUGLAS W. JOHNSON, of Columbia University, is a Major in the U. S. Army and expects to leave for France shortly.

M. L. REQUA is in Washington engaged upon work of the Petroleum Commission.

J. B. TYRELL, of Toronto, has recently returned from England.

THE service flag of the American Institute of Mining Engineers shows 546 stars.

THE report of the Geological Committee of the National Research Council indicates that materials and facilities for rapid road and fortification construction have been rendered available for use of the Engineers Corps by a survey of a 10–25-mile strip of the Atlantic seaboard. Immediately available materials were mapped and tabulated, and the reports cover—(*a*) topography, climate, and geology; (*b*) quarries, sand pits, piles, and stone walls; (*c*) road machinery and engineers capable of undertaking road and bridge construction; (*d*) maps representing the information cartographically. A committee on war minerals has been organized which has taken a census of minerals required in war preparations. Surveys have been started for gathering of data and exploitation of new ore bodies.

TOPOGRAPHICAL and geological maps of the cantonments and environs have been initiated and provision made for instruction in geology and physiography in the training camps. The committee is: J. M. Clarke, chairman, W. W. Atwood, C. P. Berkey, A. L. Day, B. Dean, F. W. DeWolf, W. O. Hotchkiss, E. B. Mathews, J. C. Merriam, R. A. F. Penrose, Jr., H. F. Reid, C. R. Van Hise, C. D. Walcott, J. B. Woodworth.